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THE PLACE OF VALUE IN A WORLD OF FACTS

THE WILLIAM JAMES LECTURES ON PHILOSOPHY AND PSYCHOLOGY *were established at Harvard in 1929 from a bequest of the late Edgar Pierce. The purpose of the Lectureship is to honor the memory of William James and at the same time provide public lectures and informal instruction by an eminent scholar not permanently connected with the University. Professor Köhler's lectures were given as the third series on this foundation in the first half of the academic year 1934-35.*

The Place of Value In a World of Facts

By
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LONDON
KEGAN PAUL, TRENCH, TRUBNER & CO., LTD.
BROADWAY HOUSE, 68-74 CARTER LANE, E.C.
1939

PRINTED IN THE UNITED STATES OF AMERICA

TO

RALPH BARTON PERRY

Preface

The purpose of this book is philosophical. It differs, however, from many other philosophical books in the degree of attention which it gives to certain parts of science. In recent years serious doubts have been raised as to whether, in its present course, science will be able to contribute much to the fundamental issues of mankind. Are we to infer that the philosopher and the scientist should live and work each in a world of his own? Actually, no boundaries separate the problems of one from those of the other. Thus, if there be no contact, something must be wrong either with philosophy or with science, or, perhaps, with both.

This conviction need not lead to a Positivistic attitude. It has not done so in our investigation. Never, I believe, shall we be able to solve any problems of ultimate principle until we go back to the sources of our concepts,—in other words, until we use the phenomenological method, the qualitative analysis of experience. In this our Positivists show scarcely any interest. They prefer to deal with concepts which have acquired a certain polish in the history of scientific thought, and they think little of topics to which these concepts cannot be directly applied. In the present investigation a less conservative procedure has become necessary, because at several points no adequate scientific concepts have been available. Positivism may not approve of our attempt on such occasions to gain clearer notions through phenomenological analysis.

There is one lesson which the philosopher might learn from the scientist, It is in the nature of philosophy to aim at the solution of general problems; but it is in nobody's power actually to achieve that much at once. Those who nevertheless try to do so seem invariably to overlook essential aspects of their subject-matter We feel no confidence in their conclusions. I wonder whether, with more patience, philosophical thought might not in fact advance more quickly. Such patience would assume the characteristic attitude of research. It would be realized that, as a rule, partial and preliminary problems have to be thoroughly mastered before general solutions can be successfully attempted. Occasionally one might then hear a philosopher saying: 'Up to this point I now know my way; but I am not at all sure what I may find around the next corner. I regard this as natural; because, like science, philosophy proceeds by steps.' I do not wish to imply that philosophers should lose their interest in general solutions of general problems. On the contrary, I believe, merely such steps should be taken in philosophy as seem required in preparation for those final achievements. In our present situation, however, the philosophical spirit might be more genuinely expressed by work which thus prepares general solutions than it would be by any premature endeavor to give these solutions themselves.

I will confess that in this remark I am partly trying to defend my own procedure. Time and again the present investigation has left scores of related questions entirely unanswered. As to the main problem there has been, I hope, some progress; but I have been unable to give a final solution in general and in simple terms. In the present sense of the word, therefore, this may not be a philosophical book.

I feel very much indebted to Harvard University for the honor of having been invited to deliver the third series of William James Lectures.

To Professor R. B. MacLeod I wish to express my gratitude for the untiring patience with which he has tried to give my text a greater resemblance to English. If this task could not be quite satisfactorily accomplished it is my fault, not his. To Professor M. Mandelbaum my thanks are due for helping me with the correction of proofs and for preparing the index, to my wife for her aid as an indefatigable typist.

This book is dedicated to Professor Ralph Barton Perry as a sign of my friendship and admiration. May I hope that when reading it he will not regret that it bears his name.

WOLFGANG KÖHLER.

November, 1938.

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CHAPTER I

The Case Against Science

When I was a young student in Germany, science and all the other branches of human knowledge were held in highest esteem. Their value was too generally acknowledged, it seemed too self-evident to require the support of any particular argument; and there were practically no people who would have dared to discuss it critically. It belonged to the noblest tasks of society and of the governments to support all forms of research. Even children were given as much knowledge and as much training in intellectual operations as seemed compatible with their capacities; because a state of ample information was so obviously desirable.

In this respect as in others the atmosphere has now changed tremendously, not only in Germany, but also in other European countries. Immediately after the war, it is true, the young people would crowd the lecture-rooms of our universities, more eager to learn than they had ever been before. It is not known whether they became disappointed by what they heard there. But soon one could hardly fail to observe that both the professors and their work no longer enjoyed the customary appreciation. Gradually signs of a critical attitude became visible in several quarters, and what might have been a temporary lack of interest proved to be a growing doubt as to whether all learning whatsoever deserved unconditional reverence. A number of years ago a popular German magazine went so

far as to ask several outstanding men of science about their opinion concerning *die Krise der Wissenschaft*, the crisis of science. And, since the term "Wissenschaft" has a more general meaning in German than "science" has in English, the inquiry of the magazine did not mainly refer to particular difficulties which had arisen in physics; rather the experts were expected to give their views about the value of learning in general, confidence in which appeared to be severely shaken. Of the answers which those eminent men published in the magazine, I do not remember much more than that they were quite different from each other; a few professors seemed to be almost as sceptical as was public opinion; not a single answer was, I believe, a fully convincing, frank confession of faith with striking arguments to support it. This, of course, must be due to the fact that the experts had been taken unawares. Perhaps it was not altogether fair to ask them suddenly such general questions. On the other hand: Why had the magazine inquired? It seemed to presuppose that the average reader regarded *die Krise der Wissenschaft* as something obvious. Since I happened to know one of the editors I asked him whether he also had lost confidence in the power of learning, and, if so, for what reasons he had become sceptical. Although my friend has had a university education, he is far from being a professor. If I give a condensed account of his statements here, I do it with hesitation. For my academic habits, his interest in what he called essential problems was far too impetuous, and a reader with academic standards will doubtless agree with me that to put questions of principle so crudely in the foreground is not a sign of very good taste. Even so, a strange form of European uneasiness, as I observed it in

1930 or 1931, is strikingly expressed in his curious complaints.

"Yes," he began, "there is indeed a widespread feeling that something is wrong with academic learning. And it is probably not the worst people outside the universities who during recent years have adopted a critical attitude. In a way their dissatisfaction is connected with the unusual sequence of troubles which one after another have disturbed Europe since 1914: the war, circumstances connected with peace-making, inflation, economic and social unrest, tremendous difficulties in government. Just now, as people are beginning to be more hopeful, there has come the world-wide business crisis and appalling unemployment. Is it surprising that they are growing restless? We meant well, they say; we tried to remain confident; if what we had achieved broke down we built it up again. But why is it all so useless? Why ever more trouble? What is behind it? They go to the professors of social science and to the historians. They ask these men about the dynamics and the deeper meaning of historical developments. They want to know whether such events as this chain of misfortunes are brought about by anonymous historical forces, or whether they can be traced to definite mistakes of responsible agents.

"What, do you think, do the professors answer? After some deliberation there are always the same remarks about the necessity of distinguishing between questions of solid fact with which the authorities are actually occupied and problems of value, of meaning and of moral or metaphysical interpretation, with which a sober discipline refuses to deal. One professor said: 'O, politics!'—he smiled in a tired fashion—'it is certainly a muddle. You cannot imagine how consoling it is to fall back on pure research.

The second edition of my "Dutch Immigration Into Prussia During the Eighteenth Century" is just about to appear. I have unearthed entirely unknown . . .' But his interviewer left him, quite unconsoled, his heart even heavier than before. As a matter of fact I know of no such interview that ever had a satisfactory end. All the professors have their little farms which they are highly skilled in cultivating. What grows there fills their minds completely. But every one of them has been careful to erect a fence against that vast, uncharted country beyond his farm in which we others try to find our way and cannot. I have an uneasy feeling. What will happen if all this special research is taken so seriously by those who do it, while the essential questions of mankind seem almost to be regarded as obnoxious? Professors fairly wince when they hear such questions mentioned; at the first opportunity they retire each to his study, his Dutch immigration and his 18th century. They had better be careful, however. It is not merely little farms, it is the geography and the law of the open land which the people want to hear explained. If the professors do not care, mark my word, other teachers will emerge and will be followed, whatever you may think of their explanations and of their law.

"I mentioned a few typical questions by which the people are greatly disturbed. There are others which they ask with the same eagerness, but which the professors never answer. 'Any research,' I heard a great teacher say, 'is valuable in itself. One cannot criticize it merely on the ground that it has no consequences in a wider philosophical field.'

"I doubt whether there is such an intrinsic value in any learning whatsoever. However that may be, I do not

see why the term 'philosophical' should be used in this connection. It is not speculation about remote questions, it is our understanding of certain empirical matters to which the professors fail to contribute. Or is the nature of historical developments a metaphysical problem while that of physical or biological developments is a problem of science? When erecting fences around your particular farms you do not only exclude metaphysics, but also a good deal of human experience,—unfortunately that part of human experience insight into which is most urgently needed. And yet you shrug your shoulders. This, you say, is not your business. You do not feel responsible for what happens outside your fences. I wonder whether you are at all interested in our predicament.

"I must repeat, however, that there is more than mere indifference in your attitude toward our problems. Why this strange impatience in the faces of professors when we ask our questions about man, society and history? Not a few professors will tell us in so many words that they do not wish to be molested by questions on such topics, and some are frank enough to admit that they dislike these topics themselves. We laymen do not understand this attitude. We fail to see any reason why learned men should assume an attitude of disdain with regard to certain subject-matters. Was the world created to fit a set of given scientific methods? You seem to presuppose just this when you look down upon some phases of creation, because their appearance is not that of neat scientific material."

Although I was seriously annoyed by the crudeness of this attack I suppressed my resentment and calmly gave the man all the explanations which his *naïveté* made obviously necessary. "You are right," I said. "Everyone of us has his special domain in which he feels, and probably is,

competent. He refrains from trespassing on what you call the country beyond. But it is easy to see the reason for this reserve. In the course of more than two thousand years, first mathematics and then one discipline after another have learned to solve their problems properly. A standard of achievements has thus been created of which, for instance, Greek philosophers were quite unaware. Experience shows that concentration on a limited field is necessary if our work is to measure up to that standard. Nothing comparable with this standard has ever been attained when people, instead of cultivating certain 'little farms,' have tried to solve those general problems about which you and many other contemporaries are so concerned. Such failures are sufficiently common in our own time and in our immediate neighborhood. In a way you need not complain. A host of writers feel called upon to tell us what the essence of man is, how the events of history are brought about, and what course history will have to take in the near future. They interpret, they prophesy, they attribute meaning to history or they deny it, all to their heart's content. When we try to read such literature we find that even the darkest language of prophecy fails to hide the shallowness and the arbitrary character of the underlying ideas. No actual knowledge—which nobody has of such matters—merely subjective opinions and the general *malaise* of our time, make these men write. All their great words about your essential problems of mankind have, however, at least one effect upon us: Since there is no substance behind them we are by now thoroughly tired not only of such literature as such, but also of its alleged subject-matter. It is not a bad but a good symptom if most professors refuse to join a chorus that sings so consistently out of tune. When asked to choose

between writing badly about the greatest questions and well about more modest topics we prefer the second alternative. It is perhaps a pity that no valuable statements can yet be made about your major human issues; but by mere wishing or by sheer will power you cannot found a new domain of knowledge, however desirable, for which the time has not yet come. Galileo observed how stones fall to the ground. What a trivial occupation! But he was right. His observations led to the foundation of general dynamics."

I thought that this would silence him. But in the manner which such stubborn people have he began at once to turn my own arguments against me. "I have seen some of those writings," he answered, "to which you allude, and I do not like them any better than you do. But what is responsible for their existence and for the unwholesome influence which they doubtless exert? It is the academic indifference and the disdainful attitude of those who ought to clarify our thought in such matters. In prolonged distress most people are unable to live without a frame of reference, an interpretation of human life, which would make their existence meaningful in spite of so much misfortune. If the professors who have the necessary training and the equally necessary intellectual discipline do not give them what they need, these people will naturally turn to surrogates, to *Ersatz*. Strange food will be eagerly swallowed in times of famine. This may have deplorable results. If such consequences appear in our case we shall ask the professors why they did not act while there was still time. Plato intended to select the governing body of his ideal state from the learned class. He must have been thinking of a different kind. The professors of our time shrug their shoulders much too often. They do not see

that with the great power of the skilled mind they also assume a tremendous responsibility.

"I am glad that you mentioned natural science which, as you rightly say, does not confine its investigations to little farms; far be it from me to belittle the great advances which have been made in physics, in chemistry and in certain branches of biology. So long as our attention remains focussed on the material knowledge of these disciplines we shall feel inclined to celebrate the irresistible force of the scientific mind. Personally I think quite as highly of the services which such research has rendered to hygiene and to medicine. There, for once, science has done a great deal for humanity. If I refrain from intoning at this point the customary hymn in praise of technical progress in human communication, travelling and so forth, I do so for a good reason. Agreeable though this special form of progress may be in several respects, it also deflects the attention of those who enjoy it from much more essential issues of man. Gradually they become accustomed to measuring the 'standard of living' of a people in terms of mere material comfort; and they forget altogether that the same development contributes little and often is even harmful to the standard of living in a higher sense of the word. On the now accepted scale, neither St. Francis nor Luther had very much of a 'standard of living'; yet it may be the great danger of our particular industrial civilization that it neither gives birth to Luthers nor accords sufficient thought to what a Luther of our times would regard as his main business.

"But let me return to natural science. Here we see indeed what real knowledge is. The more we study natural science, however, the more striking will the contrast be between such achievements and our complete bewilder-

ment in all essential human matters. Take what the social sciences have to offer and compare it with any part of natural science. What an appalling difference both as to available methods and as to the confidence which we may have in the results! I know, they give us many historical facts and much statistics in economics and in political science. But do such data lead to well-founded insight into the dynamics of economic and of political events? I need not answer this question: You answered it for me when you said that nobody has actual knowledge of such matters, that no valuable statements can yet be made about the major human issues, and that the time has not yet come to found this new domain of knowledge. Here you went much farther than I had done. I pointed out that for some reason you do not like to deal with our problems. Although I had some suspicion that, even if the professors wished, they would not be able to investigate such problems, I preferred to keep that suspicion to myself. But since to my surprise you admit the fact spontaneously I may as well be equally frank. Obviously this is the situation: The general name of science or knowledge is given to widely different occupations. A few of these are concerned with matters which are comparatively clear and simple. Such is the situation in mathematics, in the sciences of inanimate nature and in certain parts of biology. None of these disciplines tries to solve problems which refer to the essential characteristics of man, to the dynamics of society and of history. In this respect we have made a most depressing discovery: Wherever the scientific mind tries to handle *these* topics it loses its bearings; its methods fail to yield any valuable results, and mere opinions take the place which in real science is occupied by knowledge. Evidently all these problems have one element

in common which makes them inaccessible to your technique. This common element is man. Whether considered as an individual or in groups, i. e. in society and in history, it is man to whose nature you are unable to do justice. There is something in him which you cannot conquer by procedures which are quite successful in present natural science. For this reason your achievements begin to sink to a much lower level when your thinking merely approaches human affairs. Until recently we believed that the scientific mind could clarify any subject-matter listed in the university-catalogue. When in the present period of distress we found the professors unable to answer our questions, we began to discover that in certain departments the customary technique of science is practically powerless, and that nobody knows how to find new methods which would apply in these fields. On the *program* of these departments, man is the main item. Unfortunately he is *only* on the program. Science properly speaking achieves insight into its subject-matter. This can be done so long as science keeps away from man, i. e. within a limited zone of secondary subject-matters. In departments which are supposed to study human life, discussion of this program apparently serves as a substitute for actual insight and factual knowledge.

"Do you realize what a terrific disappointment this discovery meant for us? In this sense, if in no other, there is a *Krise der Wissenschaft*. It is a crisis concerning our confidence in your work. A similar crisis occurs in a child when he discovers that his apparently omnipotent parents are often quite helpless and weak, that he cannot rely on their support when things become very serious. Thus, in our present troubles, we were badly in need of a frame of reference that would give us an orientation, and we ex-

pected it from science. We know now that you have no such frame and no orientation yourselves.

"Nor was this our only sad discovery. Once made suspicious we began to see other facts which had previously escaped our notice. Why are the people in such bitter need of a new orientation in our days? There have been periods of quite as much distress in earlier times. But in those periods the population of Europe had general convictions which gave it strength to stand the strain patiently. There are no such convictions now. Why? Because of the tremendous destructive power with which during the last few hundred years learning and research have fought any stable mental orientation that existed before the era of science. At present we see science unable to *give* us such a frame of reference. Can it be a mere accident that, in the past, science has tended to destroy any conviction to which it found mankind clinging, and that at present it seems eager to destroy what may still be left?"

By now I began to feel considerable sympathy with those professors who, as he said, did not like such conversations about the "essential problems of mankind." It was perhaps unfortunate that no competent representative of the social sciences happened to be present. Of course an expert in these fields would have demolished his criticism in a few sentences. Incidentally, I wondered why he had not mentioned psychology when speaking of disciplines to whose subject-matter the basic issues of man belong. Being a psychologist myself, however, natural pride and a sense of caution forbade me to ask for an explanation. Laymen have often strange ideas about the tasks which they believe psychology ought to solve. But I could not possibly pass in silence over his startling assertion that science exerts a destructive power on the general orientation of

mankind. In what sense, I wanted to know, could a steady increase in valid information have any destructive effects?

The man seemed to have been waiting for this question. It led at once to new charges against the scientific attitude. "Your discoveries," he said, "do not directly refer to those human affairs in which we are mainly interested. And yet every major movement in science changes the appearance of the environment in which man lives. Curiously enough after any such movement this environment seems to look colder and darker. Now, our view of ourselves is not wholly independent of the illumination in which we see the world around us. If a shadow is thrown on our environment, our own mood changes correspondingly, as it were by reflection; we feel cold, and our own mind seems to have darkened; it seems no longer to be the mind that it was before. As a matter of fact this uncanny change is becoming more and more apparent, the more rapidly science extends its knowledge. But even the very first steps of science had just the same effect on man. I have no wish to discuss the validity of those scientific doctrines which alter the appearance of our environment and, thus, of ourselves. So far as their strictly scientific content is concerned I feel compelled to accept them for the most part as definitely established. You do not see how under these circumstances any criticism remains possible? Let us consider some historical facts, the history, so we may call it, of *Nothing But.*

"Before there was any science nature appeared to man as akin to himself. Things were of his kind, and events resembled his own activities or sufferings;—it is still so among those tribes which we call 'primitive peoples.' In consequence of it, intimate relations obtained not only among the most varied parts of nature, but also between

man and his environment. Although such relations were often regarded as dangerous and some as terrifying, even in this case they were at least familiar in type and to that extent understandable. When science developed, this view of the world was gradually destroyed. Nature, so the physicists say, bears little resemblance to man. Nor are we allowed to believe that in the relations between one and the other there can be any intimacy. Those particular interconnections which the primitive view finds among concrete parts of our environment are with few exceptions non-existent for the scientific mind. To be sure, the abstract laws of physics apply generally; but *in concreto* there seems to be little coherence in the world. It is loosely knit. Its primary material are minute particles, and the main principle which science recognizes in their behavior is mere chance. This in itself is a long story. So much progress in our appreciation of the world could not be achieved in a few years. Galileo for instance, was entirely unaware of the fact that nature is ruled by chance and thus escapes all understanding. It was practically our generation which made this final discovery.

"If this story is long, it is also consistent in that all its statements are negative: no resemblance, no intimacy, no coherence, and not even causation. Within this world there is little that can interest a human being who is not given to the study of abstract rules as such. It is difficult to see how he, entirely a foreigner, could ever appear in such an environment. We must excuse him if he feels lonely, cold and bewildered when he learns that this is his situation. To make things worse, science regards him with a hostile eye, because his superstitions and his anthropomorphic tendencies had given the world warmth, color, intensity and sense. Thus one difficult step after another

became necessary until the truth was fully and satisfactorily revealed, that nature is nothing but tiny inaccessible particles, and that their law is nothing but chance, the ways of which nobody can understand.

"What I have to add serves merely to complete this picture. Directly the work of science refers to nature, not to man; and so does the Nothing But in terms of which science tends more and more to express what it regards as its modern attitude. But there are two ways in which this attitude must influence the view which man has of himself. First, the shadow of Nothing But will soon fall upon man simply because he has a place within nature. Secondly, in his work on nature the scientist discards a great many characteristics of experience because, according to him, they do not represent the properties of nature, but merely those of human perception or even imagination. Whenever thus some attribute of our experience is rejected by those who build the edifice of objective physics, this attribute seems justly labelled as nothing but a subjective ingredient, as a disturbance in the path of science.

"Copernicus' discovery is a good example of the way in which man's location in nature makes Nothing But applicable to himself. This celebrated change in the description of astronomical facts had the very strongest emotional effects on mankind: The home of man is not the center of the universe. This planet is but a tiny speck of matter in an altogether unimportant astronomical position. He crawls along its surface. It is therefore sheer anthropomorphism to attribute any particular value to the aims of mankind. If Nothing But is a good term to use in reference to the earth, then it is even more so in reference to its little inhabitant. I do not remember how many times I found modern and consequently disillu-

sioned writers alluding to the miserable place which man has on the map of astronomy. I read: 'We told you so,' between their lines, even if they did not print it.

"Of the sensory qualities of perception we speak in terms of Nothing But for the other reason: the very first attempts to think about nature in the spirit of science led to their elimination. Although outstanding features of our environment, as it naturally appears to us, and altogether essential for its aesthetic character, they have none the less no place in the scientist's world. They are nothing but subjective phenomena. No physics is possible until this veil is removed. Among man's belongings this was the first to lose its value because it had no value in science. Observe how sure we are to hear from science statements which from a human point of view are negative under all circumstances. Objective nature is a world of Nothing But; because such are its actual properties. A good deal of human experience, on the other hand, falls under the same category, because it fails to resemble that objective world; in other words, just because in this respect it is not Nothing But enough. 'Nature is a dull affair, soundless, scentless, colorless; merely the hurrying of material, endlessly, meaninglessly,' [1] and thus it is no good. Human experience is not dull, it is full of sounds, scents and colors; it has ends and it has meanings. But then it, too, is no good; such things disturb the physicist, because they are not objective; they are nothing but human.

"Space and time, as we conceive them, had soon to share the fate of the sensory qualities. What the physicist calls space and time is, so he warns us, an abstract and an enigmatic scheme to which we cannot transfer the rules

[1] I found recently that these words are quoted from A. N. Whitehead, who does not of course share the opinion which they express.

of spatial and of temporal experience. Like the sensory *qualia* such experience is nothing but a subjective phenomenon.

"It almost goes without saying that the so-called tertiary qualities of our environment have, if possible, even less value. How should there be any power in lightning and in thunder? How any tenderness in a spring day? How sadness in a rainy afternoon? The scientists do not even take the time to formulate it expressly; so obvious does it seem to them: there are no such things in nature. To be sure, properly speaking there are no such things even in human perception; people are foolish and cannot resist the temptation to project upon their wholly indifferent environment what is so clearly nothing but their own emotional reaction to such neutral percepts."

I looked at the man speechless. Would he prefer ignorance to truth, if truth were uncomfortable? Would he choose superstition instead of well-established knowledge, because for the former the world is full of intense and familiar human characteristics? If ever there was a reactionary, here I had one before me. But he merely smiled a little and went calmly on: "I know exactly what you are thinking. Wait until I explain. But first let me tell you a story.

"In a great museum of natural history I once saw a curious exhibit. On a shelf there was a pile of cubes. The sizes of these cubes represented the amounts in which the different chemical elements occur in an average adult human being: oxygen, hydrogen, carbon, nitrogen, and so forth. On each cube there was an inscription which gave the current market price of the substance in question, and on a special table all these figures were neatly added. The sum was, I believe, slightly more than $63, about

one half the value of a medium-size refrigerator. Although by now prices may have changed, just as have those for other commodities, the figures were at the time doubtless perfectly correct, both as to pounds and as to dollars. What, then, was wrong with the exhibit? *Can* anything be wrong with correct statements?

"Yes, this is perfectly possible, and nothing occurs more frequently. It is also a correct statement that approximately 70% of a human being is common water. And yet, if this happened to be the only information about man which would reach the inhabitants of another planet, these creatures would almost certainly be led to an entirely erroneous conception of ourselves.

"Some objects are essentially *structures*, inasmuch as their parts depend upon each other for their existence. In all the more interesting objects of this kind, however, such interdependence has, let me say, a *direction*. Certain parts are more strictly indispensable for the existence of others than the latter are for the existence of the former. I might say the same of function; as a matter of fact, with such objects it is extremely hard to distinguish clearly between structure and function. At any rate function has in these cases an hierarchical order no less than has structure. The organism of a mammal belongs to this class: its parts are interdependent, but in such interdependence there is an hierarchical order. The heart, for instance, is more significant in this structure than is the tail. Circulation is a more essential function than is the action of the sympathetic system whose elimination, so I read recently, *alters* other functions but does not make them impossible.

"But, I forget, is not this precisely the situation with which you are dealing when, in psychology, you are concerned with *Gestaltprobleme?* Well, it seems to me, this

same situation leads to interesting consequences in our appreciation of *statements*. The objects to which our present discussion refers may as a rule be inspected in various ways. As we concentrate upon one or another part of their structure, upon one or another of their characteristics, we shall obtain statements which may without any exception be perfectly 'correct.' And yet these statements may do more or less justice to the objects in question. The reason is this: Since there is a hierarchy in such an object itself, genuine truth about this object can only be given in a *set* of statements which, by its order, reproduces the hierarchy of the objective structure. From the point of view of entirely objective knowledge, therefore, a statement may be 'correct' in one sense and at the same time more or less *true* in another sense. Put in its right place among other statements about the same thing, it will express a fully adequate judgment; put in the wrong place it will be misleading, although in a way it may remain literally 'correct.' Again, a single correct statement about a *basic* feature of the object will in the same sense contain much truth, even though this truth remain imperfect. But a single 'correct' statement about a minor or an altogether *unessential* phase of the object will be quite ambiguous, because its meaning lacks that context in which it could become definite and fair in respect to objective structure. The former statement loses less by such omission than does the latter, because the first refers at least to the nucleus of the subject-matter, while the second hides the existence of such a nucleus.

"As a matter of fact more happens than a mere omission when single statements are given about unessential characteristics of an object. Statements are symbols of thought and of meaning. This is generally recognized, and we both

know how much has been done to clarify the nature and the function of such symbols as words and sentences. We also realize that in sentences the sequence of words is often as much a symbol of meaning as are the words themselves. Yet another step becomes necessary if the symbolism of language is to be fully understood: the meaning of a statement depends upon its 'environment.' We just recognized this fact when mentioning that statements ought to have the right place in a set of other statements to which they belong; this *place* as such has symbolic value. Even thus, however, the symbolism of language is not yet exhausted, because, if a single or a few statements are made about an object, and if no further 'environment' is given to them, such silence around our actual judgments has again symbolic value. Whether or not we are fully aware of it, such a silence conveys the meaning: those statements which we actually make are statements about essential phases of the object in question; this is what has to be said about the object. It follows that single, or a few, 'correct' statements which refer to unessential phases of a subject-matter are, when given alone, not as much ambiguous as positively misleading and, always in the same sense, untrue.

"We may now return to the museum exhibit. The presence of those chemical elements in precisely those proportions does not characterize the human organism as a living concern; for in a corpse the same elements are contained in exactly the same proportions, at least immediately after death. And, so one should think, we refer to the living organism, not to a corpse when we make statements about 'the adult human being.' For the same reason the market prices of those substances are entirely irrelevant to an appreciation of man. From this curious

point of view the corpse and the living adult have the same value. Thus the data of the exhibit, given singly as they were, conveyed untruth to the public in just the sense which I tried to explain a moment ago. They had on the other hand the great merit of contributing to the atmosphere of Nothing But in which the more advanced people want us to live. From now on, whenever I hear further negativistic statements, I shall always remember that exhibit and its implication: no more than $63.

"You will see at once that the same reasoning applies to the strange attitude of those who base their appreciation of man on the limited size of his planet and on the fact that this planet travels on a secondary route. Since the foremost characteristics of man show no direct relation to astronomical dimensions and to the road-map of the stars, why should we measure him in such an astronomical coördinate system? We do not evaluate paintings in terms of square inches, although we *can* measure them on this scale and shall then find them practically non-entities in comparison with the Sahara. Thus man *can* be seen in a merely astronomical scheme of things; and, if he appears in this scheme as almost non-existent, a corresponding statement will be 'correct.' And yet, always in that same sense, such a statement will be altogether untrue,—particularly if its negative import be transferred to essential properties of human life to which it actually bears no reference whatsoever.

"To make my point clearer, I will for a moment reverse the procedure and choose my coördinate system in such a way that it fits the outstanding characteristics of man. From this point of view, man represents an amazing level of organization; moreover, at least some men seek improvement both of themselves and of human society so

persistently that this is doubtless the most essential of their traits. Nowhere in the astronomical universe *qua* astronomical do I find anything that approximates such organization or such a need for improvement. On this scale, then, one Socrates scores more highly than all the known stars taken together. It might be said that the universe should not be measured on this scale. Perhaps it should not. But then, why should Socrates be measured in astronomical terms? It is an altogether arbitrary procedure.

"The negativists ought to be more cautious in handling coördinate systems. Otherwise somebody will come sooner or later, and will say: After all, Socrates belonged to the universe, he was a part of it. What tremendous powers, what amazing potentialities must be hidden everywhere in the universe, if this planet could produce a Socrates.

"Negativists, however, have probably just as tender feelings as have other people. I shall no longer hurt these feelings by the threat of a possible optimism. Keeping on the defence I will only say this: Many statements by which advancing science makes man and his environment appear discolored and devalued are suspect of being 'correct' statements that are not actually true in our now so long discussed sense. They refer to some properties of the world in its relation to man, but preferably to those which are not relevant for the picture of man in his relation to this world and for the knowledge of man himself. Since no other statements are added that would be more adequate, the people must believe that thus the fundamental truth about man and nature is revealed. Such belief will be enormously strengthened by the fact that those statements come from natural science which is supposed to offer better truth than does any other source of information. In this way they learn to speak in terms of Nothing But,—

not only about nature, but also about themselves. The information, however, on which this attitude is based, belongs, if I am not mistaken, on the same shelf on which I found the cubes and $63 as the price of a complete adult."

I do not like rhetoric very much; but I cannot deny that something in these last remarks of his made a certain impression on me. I refer particularly to his distinction between "correct" statements and the true knowledge which is obtained only when statements are put in their right places. If such remarks had come from men like Wertheimer or Koffka, I should not have been surprised. But it did surprise me to hear them from an outsider. Perhaps there was more in the man than I had expected. Since, however, his own opinion of himself seemed to be high enough, I did not say anything about this point. It was probably more appropriate to tell him, that as yet the only concrete application of his principle which he had given was to Copernicus' discovery; and that he would still have to show in what sense other, perhaps disappointing, theses of science were "correct," but not in his sense genuinely true in their reference to man.

When he heard this, he said: "Once more, Professor, you have given the answer yourself. Let us put two and two together. May I repeat: Misleading and therefore untrue 'correct' information will result when we make statements about unessential phases of an object, while such further knowledge as refers to the fundamental properties of the same object is never added. Now, there may be several reasons for not adding such further statements as would give the others their right peripheral location. In the first place, we may not wish to make such informa-

tion known, although it is at our disposal. Or, in the second place, we *cannot* add those more essential statements, simply because we lack the knowledge that would make them possible. And this is actually the situation in which the scientific mind finds itself in our case. You were, I believe, particularly convincing when in the beginning of our conversation you said that no valuable statements can yet be made about the major human issues. If this be true I have little to add. I can only conclude that those statements of present science which in a way refer to man must all be understood with this warning: Whether or not such available information will finally prove to have any weight in our appreciation of man, and what ultimate interpretation we shall have to give to it, these questions cannot be answered until a way is found to deal adequately with the fundamental phases of human life, and thus to establish genuine truth about it.

"I am inclined to go a little farther, and to apply the same principle to the sad picture which the scientists give us of nature as such. Sometimes I cannot help wondering just why practically all the statements of modern science which seem relevant to our view of the physical world tend to have the same negative character, in that they make nature appear less and less understandable, more and more foreign to our eyes. Could this strange regularity have causes which are not altogether objective? Science is not capable of doing justice to man; his main traits do not fit into its standardized categories. With this premise, how could the scientists do justice to any phases of nature in which it might resemble man? Such facts would escape their attention or, rather, by the methods which scientists use they would be rendered invisible; and this would happen so regularly that after a while every

scientist would be convinced: there are no such facts. In this case the present interpretation even of the physical world might conceivably be open to revision,—not as though the physicists' present statements were incorrect, but in the sense that in the future certain characteristics of nature might come to the foreground and be more adequately interpreted, which are as yet not seen in their right places. If this should ever happen, Nothing But might at the same time lose some of its influence on our view of the physical world.

"But let me come back to the history of negativism in science. This history has one chapter of which I have not yet spoken, a chapter that has much to do with your own field, with psychology. Once in a while a great movement in science does refer to man more directly than was the case in our previous instances. Perhaps I said too much when I contended that science does not know how to deal with man. In a way it knows only too well how to do it. And we can predict what must happen on such occasions: Science will misrepresent the very essence of man in order to adapt him to its system. The outcome will be ever more statements in which 'nothing but' are the predominant words.

"With this expectation actual facts are in complete agreement. When some of Darwin's followers began to apply his particular theory of evolution to man as an apparently reasoning animal, they were led to statements in which the very nature of reasoning and of insight was implicitly negated. I have no objection to the idea of evolution. On the contrary, I believe that few other ideas have widened our horizon as much as has this change in our biological perspective. I am much less enthusiastic about those special principles of evolution which, accord-

ing to the later Darwinists, are exclusively responsible for the change of existing species and the emergence of new forms of life. Since, however, we have as yet no better theory it seems useless to discuss this point at present. On the other hand, the application of these principles to the mental life of man appears to me as both the most curious and the most dangerous instance of Nothing But with which I am acquainted.

"Human thinking is from this point of view a particular function of the organism, just as are the processes of digestion or of circulation and respiration. I can accept the first half of this statement, but in its second half I recognize premises which lead to preposterous confusion. Digestion, for instance, depends on the existence of some major structures, the mouth, the stomach, and so forth, and on the presence of some glands, or chemical devices, whose products make adequate assimilation of foodstuffs possible. Digestion is not the same process in all the various species of the animal kingdom, and the modifications which in this respect occur from one species to another have to be explained by a theory of evolution. In the more orthodox form of Darwinian theory we hear about two main factors by which the development of species and of their functions is said to be achieved: chance variations within the germ-plasm, which lead to subsequent and corresponding variations in the structure of the adult organism, and the given properties of the environment, by which 'unfitting' variants are eliminated. The only primary changes which can be brought about in this manner are obviously changes of structure, of anatomical conditions. Organs become stronger or larger, new organs are gradually formed, as one accidental variation follows another, and as 'wrong' variants die out soon. If, therefore,

digestion changes from one species to its descendants, accidental variations in the germ-plasm must have led to changes in those particular anatomical structures which determine the course of digestion.

"Human thinking depends directly upon the function of the human brain, more indirectly upon the function of other organs. Nowhere else in the animal kingdom do we find any thinking that approximates the level of human thought. Human thinking is thus a product of evolution. If the Darwinian principles of evolution are generally applicable, they have to be applied to this case. This has been done, and here is the result: Just as do other parts of the organism, so properties of the brain change when accidental variations occur in the germ-plasm of animals. Since such changes occur blindly, their influence on function will for the most part be detrimental. Animals will die early when their brains and consequently their mental processes function in such a new way. Occasionally, however, such a change of brain-structure and of mental function will lead to behavior that happens to fit the nature of the objective environment even better than did the previous behavior. The fortunate owners of such improved mechanisms will, of course, live longer and have many children, whose germ-cells will then play the same game of accidental variations on a slightly higher level. In this manner—there is no other way for them—the Darwinists explain the appearance of anything like thinking among the higher organisms, and particularly the emergence of human reasoning.

"The implications of this theory were not at once realized. It took the people some time until they saw that what had happened meant a radical devaluation of the notion of thinking. I hope that a few words will

suffice to make this perfectly clear. Where, indeed, is that 'fitness' located of which the theorists speak, and which, they say, makes certain animals with certain brain-structures and with corresponding mental functions survive in the struggle for existence? Such 'fitness' is not located in the animal itself or in its functions as such; it is a factual relation between, on the one hand, forms of behavior that follow from those structures or those functions, and, on the other hand, the actual properties of the environment. If any mental function is called 'fitting,' such an expression is, from this point of view, always an abbreviation. It does not refer to the function itself, nor to any intrinsic trait of it; it merely refers to the external results of this function in their relation to the equally external environment.

"Or let me put it this way: The theory regards normal mental processes as causally determined by two factors. First, there are the general laws of nature, which apply to the organism as they apply to other systems. Secondly, there are those anatomical structures which give such laws a particular form of application in the case of the brain. These conditions determine the course of mental processes just as strictly as other conditions determine the course, for instance, of digestion. As necessary facts they are in no essential way different from any other facts. More particularly, as facts they are intrinsically by no means 'better' or more 'fitting' than are those mental functions which lead in other variants to early death. Normal thinking, it is true, prevents such early death. But precisely this is the only reason why we call it normal. Otherwise we have no right to call one function more 'normal,' more 'fitting' or 'better' than any other. Thinking is 'right'

in exact proportion to its survival value, and in no other sense whatsoever.

"By now it will be evident that according to this theory we all suffer from a curious delusion. We believe that in a given mental situation we draw this conclusion and not another, that we decide for this action and not another, because this conclusion and this decision appear to us as directly asked for by the properties of that situation. Most particularly is this the case in mathematics, and in mathematical situations which do not seem to have any relation to our chances of survival; but it is also true of a great many other situations. It may not always be easy to see at once and clearly just what a given situation demands, and errors will occur quite often. But even then there is at least awareness of this most characteristic trait of thinking: we feel that we *ought* to find that particular decision which will have the *right* relation to the given conditions of our problem. In recognizing the relations which make one thing demanded in a constellation of others, man exhibits what he calls *insight*. He does it in purely intellectual activities, as when he says that this follows from that, or that these two views are incompatible. He does it in his moral judgments when he says, for instance, that this was not a fair trial, or that wanton cruelty cannot be tolerated. Again, if he says: The weight of those columns is altogether out of proportion in a building of such light elegance,—insight is once more the outstanding characteristic of his statement.

"In all these cases fitness of one thing in its relation to others, or also lack of such fitness, seems to be given or to be found in direct inspection of certain situations. When insight refers to practical situations it seems to us itself a most practical gift, because its decisions tend to do

justice to the nature of such situations,—which is merely another expression of the fact that it is insight. To this extent insight as such will also have survival value. Is it this, however, that our theorists have in mind when they claim that by chance variations and by the elimination of unfitting variants man has acquired fitting modes of mental activity? By no means. Take it the other way around, the theorists would say, and then we shall agree. When man experiences that certain sequences of mental events lead to behavior which has, as a matter of fact, survival value, he calls such mental facts 'right.' It would be better, however, if instead of this word some neutral symbol were being used for those particular events; because the word 'right' leads to the unfortunate notion that some combinations of mental facts are intrinsically better than are others, and that mere inspection of a material may disclose whether or not its parts fit each other. Fitness means nothing but survival value. That direct awareness of fitness, on the other hand, which you call insight, and which seems to you to be independent of a possible survival value,—such insight does not exist. It would destroy our purely scientific conception of human mental processes if, instead of being a factual and external relation, fitness or its absence were directly recognizable in given situations. In a situation any particular thing is intrinsically as good, or rather, as indifferent as would be any other thing by which we might replace it. And so long as *mental* situations are being considered from a scientific point of view, the same rule will hold in *their* case. Science does not deal with values.—

"I promised to show that in dealing with the essential characteristics of man science will misrepresent such traits entirely, and will thus adapt them to its system. I think I

have kept my promise: Insight, so science declares, is a delusion; when critically examined it proves to be nothing but the fact that the external consequences of certain mental events have survival value.

"I call such a theory *naturalistic;* because it represents intrinsic fitness as an illusory appearance of certain natural and, as such, indifferent facts. I call the theory *relativistic;* because, if intrinsic fitness and insight are replaced by mere survival value of certain mental facts, our judgments on intellectual, on moral and on aesthetic matters are 'true' only in relation to the environment in which we happen to live. If this environment should change, other forms of so-called judging might become more advantageous in the struggle for existence; and what was true before, would now become wrong. For this reason the theory is thoroughly *sceptical.*

"You will not object that the Darwinists have never explicitly drawn such conclusions. It does not matter whether they actually saw and formulated the sceptical consequences of their theory. Such consequences were doubtless implied in what they actually said, and after a while such scepticism became part of our intellectual atmosphere. At any rate, if not the theorists in the field of evolution, then your own colleagues, the psychologists, made it perfectly clear, that, so far as they are concerned, science objects to the very concept of insight. It is the intensification of indifferent bonds among equally indifferent mental contents, it is mechanical associations, or—that sounds still better in an era of Nothing But—it is conditioned reflexes which explain both the development and the smooth functioning of mental processes. Insight? When we last met, you told me yourself that some American psychologists always write 'insight' in quotation

marks as though it were either an extremely funny or a slightly disreputable notion. I heard of one whose subjects never mentioned any such concept in their introspective reports, and who consequently denied that there is such a thing.

"The main issue, then, is clearly this: Is the human mind to be regarded as a domain of mere indifferent facts? Or do intrinsic demands, fittingness and its opposite, wrongness, occur among the genuine characteristics of its contents? This question, you will admit, is in principle the same whether we consider the more intellectual or the moral and the aesthetic phases of mental life. In all these fields we find the alternative: mere facts or, besides mere facts, right and wrong in a sense, that varies to some extent from one field to the others, but shows everywhere the same fundamental contrast to mere facts. Let us for the moment give the name *value* to this common trait of intrinsic requiredness or wrongness, and let us call *insight* all awareness of such intellectual, moral or aesthetic value. We can then say that value and corresponding insight constitute the very essence of human mental life. Take any major human problem, and you will find that it contains this factor. Even the word problem has an understandable meaning only in relation to the same factor. Most of all do *those* questions refer to value with which we went to the professors, and which they never answered. When, during the 18th century, other religions began to lose ground, there remained this one firm conviction, that insight—which they then called 'reason'—is the guide on which mankind can rely. However, when science tried to conquer mental life it found the notions both of value and of insight so little to its taste that it refused to recognize them as legitimate concepts.

"For this reason, Professor, your colleagues refuse to answer our questions. For the same reason they dislike our problems. Again for the same reason there is no actual knowledge about the basic characteristics of man, of society and of history. How could there be such knowledge? It would have to refer to value and to insight in practically every sound statement. Without clearness about the import of such notions no actual understanding of those topics will ever be possible. You give us correct but misleading statements about man so long as your statements refer to mere facts and not to the very essence of the human world, to value and to insight in values. Lastly, science is a destructive agent in that it tends to demolish not only this or that particular valuation of man, but even his belief in value as such, as a principle that transcends mere facts.

"Modern science has given us not merely naturalistic scepticism; it has in recent times added historical and sociological versions of relativism. Moral convictions, for instance, are said to be no more than a by-product of historical circumstances, and to vary with these. Or, again, such convictions are represented as mere factual consequences of given social structures, which vary when these are changed. Sometimes a professor of the older generation shakes his head and asks in great astonishment why the younger people of our time are so restless and so cynical. What surprises me is his surprise. When once born in the universities, the spirit of Nothing But does not remain confined to these institutions and to scientific books. Future teachers absorb this spirit in lectures and in reading. Afterwards they propagate the same spirit in high schools, both by what they say and by what they never mention. Enlightened writers do likewise when

writing in newspapers and in magazines. Thus negativism spreads through a population like an epidemic. I feel with Faust:

Nun ist die Luft von solchem Spuk so voll,
Dass niemand weiss, wie er ihn meiden soll.

"Gradually Nothing But becomes the unformulated creed of your postman, your politican and your prime-minister. When this phase is reached—and we have reached it—few people will have any stable convictions beyond their personal interests, which seem to survive even when, as values, they should also succumb. Excepting these interests there will be little that can make a man move, mostly if action should be uncomfortable or even dangerous. No principle will seem worth defending. What after all are principles? The only principle that still holds is: I want to be left alone. On the other hand, if certain people insist upon some new principle of their own, and if it looks as though resistance might lead to inconvenient consequences, why should we resist? 'What is truth?' said Pontius Pilatus, who must have been an advanced mind. We submit meekly because one principle seems to us as good or as arbitrary as any other. Without any conviction whatsoever nobody can be expected to have much courage.

"It is late, my friend, and you want to go. Come again some day, and let us then discuss what may happen to science itself, when it is found unable to deal with value and with insight. Man is not only a subject-matter of science; he is also its author. From this point of view the consequences of scientific Nothing But will necessarily fall back upon science itself. Curiously enough, science

seems still to be regarded as a genuine value. As William James once grimly remarked: According to the scientists we must be willing to see the world perish, in order that *scientia fiat*. But, if there is no other value, why should science be an exception? Science can have no more weight and sense than it itself attributes to human insight by which it is produced. Indeed, any survey of the present intellectual situation shows clearly that a sceptical interpretation of the meaning of scientific knowledge is now spreading just as fast among the scientists as general scepticism is spreading among the population as a whole.

"When in America some shallow pretence, some unjustly inflated notion is being attacked they use for such action a term that sounds rough and has no place in the dictionaries, but which is tremendously characteristic. They call it 'debunking.' Inadvertently we have in all countries begun to do something else: to debunk whatever is expressed in positive, and not in negative terms. It is like a mental disease. Unfortunately I am not a scientist and not a philosopher. Otherwise, do you know what I should like to do? I should be willing to do some more debunking. I should like to debunk the debunkers."

CHAPTER II

Theories of Value

I

I have reported about a conversation in which science was bitterly attacked by a friend of mine. Either, he said, science is impotent wherever it comes near the essential problems of mankind, or else, where it does try to handle human affairs, it tends to distort their very nature. At the bottom of all human activities are "values," the conviction that some things "ought to be" and others not. Science, however, with its immense interest in mere facts seems to lack all understanding of such "requiredness." Eventually this critic went so far as to say that science is apt to destroy its own basis and consequently its self-confidence. There is no scientific procedure without at least the requiredness of logic, the distinction between essential and unessential facts, and so forth. A science, therefore, which would seriously admit nothing but indifferent facts even in its own procedure could not fail to destroy itself.

In other respects, I surmise, there may have been too much sentimentalism in my friend's attitude. However, after some deliberation, I have come to the conclusion that in this one point at least there is something worth our attention. It might be objected that, in his bitter polemic against science, he has only set up a straw man in order to knock it down again; that everywhere in science the no-

tion of "requiredness" may be discovered as operating implicitly or even explicitly. But I could hardly admit that this would be an altogether adequate answer. There *is* a tendency to discard such a notion from science. Why else should some followers of Darwin have been so eager to explain the logical side of thinking and the ethical side of human life on the basis of mere facts? Moreover, many of us in whose writings the notion of "something being demanded" is implicitly adopted, will not be able to formulate explicitly what is meant by such a term; at least we cannot give a clear account of the relationship existing between indifferent facts and things regarded as "required." Thus, this notion has not yet a definite and well-recognized place in the system of scientific concepts.

Scientists will insist upon "objective procedure," on "careful verification," or on "genuinely scientific theory," perhaps on "the principle of parsimony" and on "consistency." Besides they will courageously defend freedom of thought, of research and of speech. Implicitly all this is accepted as valuable, as required. But the very next moment they will express their contempt of "metaphysical speculations such as concern ethics," which "cannot be submitted to the absolutely indispensable experimental test." One begins to wonder whether logic would also have to pass this indispensable test—which is itself full of logical premises. Certainly in science we are not very clear about requiredness although our work is utterly imbued with it.

Nobody can deny, however, that this same concept has played a dominant rôle in earlier phases of European thinking, and that it is still a main topic of philosophy in our time. Indeed all those philosophers who are not swept away by the flood of modern Positivism seem to agree that

the problem of value or, more generally, of requiredness, is gradually becoming the outstanding difficulty or the eminent task of modern thought. Unfortunately, if the scientists do not like to face such concepts frankly, the philosophers are far from agreeing about their nature. A survey of theories as given, for instance, by Professor Perry[1] reveals a bewildering variety of opinions. So different are these interpretations that the field of value has not even the same extension for different philosophers. Ethics and aesthetics seem to be universally included; not always logic. It is certainly not a comforting thought that even this elementary question is answered in many ways. For our present purposes we shall avoid a discussion of the problem by choosing the term "requiredness" which applies equally well to logic and to values in the narrower meaning of the word.

If we consider the theories of a few philosophers, the differences of interpretation will become obvious at once. At the same time we shall learn about at least some of the mistakes by which reasoning about requiredness is so easily led astray.

Of Plato it may be said that the whole of his philosophical system is centered around this concept. And, like my friend of the first chapter, the great Greek philosopher seems utterly preoccupied by this one idea: to give an adequate place to the notion of "something which ought to be." We remember how he tried to achieve this. "People feel moral obligations," he would say, "they recognize ideals, they speak about truth. Though they are not very clear about such 'oughts,' there must be some source even for imperfect convictions of this kind. Around all of us,

[1] R. B. Perry, *General Theory of Value*. 1926.

however, there is the empirical world of rather confused occurrences. These alone could not possibly have induced the people to discover notions like those of truth and of 'obligation.' There must then be another source of information. In fact, if you handle people with special skill, you will be able to extract from their own minds new insight about things as they 'ought to be.' This may happen, although during their actual lives they have never had this particular knowledge before, and even though they are certainly not reading it directly from any facts of outer experience. All this would be easily explained if the new insight were not altogether new after all, if it were, rather, a case of recall or remembrance—not of facts experienced in this life, to be sure, but of facts in a previous life and in a better, an imperishable world, where people were actually surrounded by things 'as they ought to be.'[2] Somehow even the facts of the confused world around us seem to remind us of this superior world; otherwise we could not understand the restlessness of the people who are always striving toward some improvement, and who know that something 'is required,' that it ought to be."

The charm of this doctrine and its incomparable prestige must not prevent us from examining how much is really achieved by it. Does it answer the question how,

[2] Some philosophers have doubted whether Plato really meant to defend an *existence* of perfect things in a world of their own If, sometimes, he speaks as though this were indeed his opinion and as though people had really had adequate perception of such 'perfects" during a previous life in that world, all this, according to their interpretation, is to be counted among the poetic tales in which he often clothed the more abstract meaning of his theories Considering the intimacy, however, which for the Greeks truth always seemed to have with genuine reality I am not sure whether we are entitled to such a radical symbolization of his very definite statements

besides indifferent facts, there may be things required? Does it render such a notion legitimate? I am afraid it does not. It only asserts that somewhere there are things in existence as they ought to be; it praises their beauty and their goodness; it points to some place outside our world where they are, and gives a genetic explanation of our imperfect knowledge of them. All this does not clarify the concept itself. In fact, if certain objects are as they should be, how do they differ from common things and facts which simply are or occur? What in their constitution, gives them this enigmatic character? If we learn that they are beautiful and good, this again does not mean more than a repetition of our problem. If they are located in a world outside our common experience, our problem, too, has been shifted outside and become less accessible there than it was before.

There is, in man, a remarkable tendency to be soothed and satisfied whenever a problem, instead of being solved, has merely been located somewhere. Take the example of neurology in an earlier phase of its development. When it had become obvious that such mental functions as speech, other movements, vision, hearing and so on are connected with the activity of definite areas of the brain, there was as much satisfaction for a time as though the problems implied in those functions had been solved altogether. Of course, those discoveries meant a great step forward. If speech, for instance, is localized in definite places of the nervous system, from now on our treatment of the faculty of speech will necessarily take another course than would otherwise have been the case. But our original problem refers to a certain group of remarkable *functions*. Even if we know where these are localized in the brain,

we do not yet understand how they are brought about in their brain-centers. We now know the location of our problem, but in its functional aspect it is still to be solved. We are easily tempted, however, to take its localization for its solution.

Between this example and Plato's procedure there is some similarity of form. Once he has declared that, in another world, the very essence of what "ought to be" may be found, no problem seems to be left; though, actually, the nature of this essence has not been clarified at all. But in Plato's case, and more generally throughout philosophy, yet another temptation is connected with the mere localization of a problem: If you are occupied with a notion to which you are ascribing the utmost importance, and if you can then locate it in a place which is by common consent beautiful and worthy of reverence, this very location of the notion may suffice to warrant its acceptance and to render it immune to question. Logically dangerous under all circumstances, such a way of thinking is less acceptable than ever in the case of our problem. Indeed, what does it mean when a place is so surrounded by an atmosphere of reverence that its very name seems to be imbued with a flavor of distinction? Obviously, this does not indicate more than that something about this place, some property of it, is eminently a matter of value, and that practically all people have this same opinion about it. Our task, however, is to elucidate this very concept of something which "ought to be." We do not solve the task, if we locate the problem in a place to which the mass of the people will, with us, ascribe a very high degree of that same enigmatic quality. We were not asked to show that our notion is extremely important—in such a pro-

gram it would be *presupposed* that some things "ought to be" in preference to others; we were asked to *clarify* this notion.[3]

In Plato's philosophy, the place where the problem became absorbed in a halo of general reverence was not very well defined. But certainly it had to be outside the confused world of common experience, free from the most confusing aspect of this experience, viz., endless change and restlessness, finally free from that relativity which is so utterly disturbing in all human decisions about what "should be." "Eternal," "changeless," "absolute," such are the terms sufficiently endowed with dignity to make us forget that "dignity" is only a particular case of our very problem.

It will be illuminating to consider one more phase in the historical development of this problem: At the time of Immanuel Kant, in the 18th century, the task had become much more urgent. Its solution, however, had also become much more difficult, since, under the impact of young natural science and under the cruel scrutiny of British Empiricism, many things which "ought to be" had begun, for a careful observer, to show symptoms of weakness and of that sickly change by which they were gradually transformed into mere facts which simply are or occur. Even then—my friend was right here—there was already so much destructive energy in human thinking that its

[3] It is one of the oustanding traits of Professor Perry's *General Theory of Value* that, over and over again, he insists upon the distinction between the problem of value as such, the generic problem, and all investigations of more particular questions about values. Very often such investigations either tacitly presuppose some solution of the generic problem, or the authors do not seem to see that there is such a problem, and that all further efforts depend upon its solution.

results began to undermine the very basis of science at the same time. According to David Hume, we remember, there is no evidence whatever that the effect which follows given causes is to be regarded as something that "ought to follow" them; it simply follows as a matter of fact. Thus causation, apparently the main principle of scientific research, seems to lose altogether the character of an "intrinsic necessity" which we might "understand."

Kant has become famous for his attempt to save the notion of strict requiredness in science and in other fields. He starts by granting at once that the mere material given to us is of a purely factual nature. Anything may occur there, nothing, so far as we can see, is required. If, nevertheless, there is real science—as mathematics, for instance, and Newtonian physics; if, besides, so much in aesthetics and ethics has the appearance of being intrinsically required,—the source of all this must be found elsewhere. And Kant is convinced that he has found the source: The human mind, in all its departments, has a structure of its own; therefore, whatever the human mind apprehends, will always be grasped according to definite rules. All human experience will necessarily appear in forms which correspond to the structure of the human mind. To that extent, although only the impact of some material will bring the mind into action, such action itself will follow its own laws which are not derived from the material. These laws, then, and the structure of the mind, expressed by them, may be called *a priori*, that is: conditions to which all experience and reasoning is subjected.

It is Kant's opinion that here we have the genuine "necessity" we had been looking for, that those and only those traits of our experience or thinking are really "re-

quired" which represent the forming power of the mind's own structure.[4]

Let us examine the alleged solution as we did in the case of Plato's philosophy. I am afraid there is no solution. Does the concept of a structure of the mind by itself elucidate the notion that certain things are intrinsically "required"? Certainly not. There is no direct relation between one concept and the other. How about the idea that such a structure will "necessarily" give its own form to all experiences of the mind? Such an influence may be unavoidable as a fact; but I cannot see that, in this manner, its consequences would become "intrinsically required." On the contrary, since the material does not seem to have any voice in the matter, and since, besides, we do not learn from Kant why the mind should have precisely this structure and no other—there is not a trace of intrinsic requiredness in Kant's "necessity" of structuralization. Seen from the side of the material, at least, this formation looks almost as arbitrary as the selection of a first name by the parents of a newborn child.[5]

It is true that as yet I have not mentioned all parts of Kant's theory. He tried to go farther by searching for an ultimate essential trait in the structure of mind which

[4] I do not overlook the fact that Kant has given two demonstrations of his theory, one which has a slightly psychological and subjectivistic flavor, and another in which he considers what is logically implied in strict science. If, however, in this second procedure certain formal principles are found to be prerequisites of science it does not follow that they belong to the structure of the mind There remains the other possibility that, to some degree at least, they are inherent in the "material" The validity of Kant's theory depends altogether upon his assumption that, in the "material," there is no intrinsic principle of order *Cf.* J Laird, *A Study in Realism.* 1920, p. 32.

[5] This is the reason why Kant's theory cannot be concretely applied. In space and time, for instance, there are so many possibilities of "formation" *a priori.* We fail to see how in a given case a selection is made from all these possibilities.

might be at the basis of all such formation. In his eyes such an ultimate trait is given in the unitary character of the mind, a necessary condition of all its form-giving power. This conception, however, is far too general for a real elucidation of our problem. Even such a simple experience as that one thing appears larger in size than another, can hardly be understood without the assumption that consciousness has a certain unitary character. This relationship, however, as also the relations of brightness, of shape, and so on, are, as such, of a completely indifferent character, mere matters-of-fact again. There are, in this sense of the term, relations between all possible objects and facts. The notion of requiredness, on the other hand, seems to contain something that does not occur at all in the concept of mere relation. Thus, the mind may have a unitary character—on the basis of this general idea alone we cannot possibly solve our very concrete and particular problem of requiredness.

These critical remarks show at the same time that, in its general outline, the solution attempted by Kant has much in common with Plato's effort. Once again the problem seemed almost to be solved, since it had been located in a particular place, although in keeping with the intellectual climate of the 18th century this was not a place outside our world. In the meantime Descartes had lived, and gradually the human mind had become an object of almost general reverence among philosophers. Located there, the source of genuine requiredness, all "oughts," seemed almost to be beyond scruple or criticism. Words like "the mind" and "*a priori*" were now almost as good a protection as "eternal," "changeless" and "absolute" had been at the time of Plato. Nevertheless, if we examine Kant's theory, it contains very little about the nature of genuine

requiredness, and it fails completely where the relation of this concept to mere facts is considered.

II

Kant's theory could not check the rise of the naturalistic tide. During the 19th century mere facts of science began everywhere to occupy places which before had been dominions of the "intrinsically required." Gradually some philosophically-minded men began to retreat from one field after another in order to defend a smaller area against this danger—the Vitalists from inorganic nature into the biological realm, one group of Humanists and some philosophers of culture still farther back into the realm of the mind and its achievements. Even this, however, was not sufficiently large a sacrifice. In the meantime the relativistic consequences of naturalism had become most threatening precisely in this field; and there were people who would go so far as to say that the principles of true thinking, of logic, were nothing but laws of real thinking, laws of nature, as it were, and therefore laws about the occurrence of certain *facts*. At this point it was, of course, a logician, Edmund Husserl, who saw the danger most clearly. He tried to save the notion of "intrinsic requiredness," in his own particular field but, very characteristically, his attempt towards salvation began with a further retreat.

Two naturalistic ideas which had proved to be fruitful in other fields were, at the time, being somewhat hastily applied as explanations of logical principles. One of them was "learning," the other "evolution." Individuals have learned to proceed mentally in certain ways, concepts have gradually been formed by the superposition of many in-

dividual cases in memory,—such would be the *empiristic* interpretation of logic and of its conceptual material. In the struggle for survival through many thousands of years those individuals would have survived and therefore could have had similarly endowed offspring who were equipped with more practical mechanisms of thinking or association, —this would be the *evolutionary* explanation of logic. Both these interpretations, Husserl found, had become so much the fashion of the day, were being so lightly proposed and accepted that hardly anybody would give much attention to the thing itself, to the concepts and logical principles which they were meant to explain. Under these circumstances, so Husserl thought, even essential traits of logical requiredness might be overlooked in the haste of naturalistic interpretation, and some of these traits might not agree with such explanations at all. This is one of the main reasons which made Husserl formulate the slogan: Back to the things themselves! And it is in this manner that his challenge is to be understood. Wait a moment, he seems to say, before you try to explain. Look carefully at a thing before you begin to hide it behind a veil of routine-ideas about learning and evolution. Try to get a full view of what you intend to explain. Otherwise you may be led completely astray. Since, however, all of us tend to be carried away by naturalistic habits of thinking, and since, in this manner, the very basis of philosophical meditation becomes mixed up with doubtful hypotheses, looking at things themselves is a difficult art which we have to develop first of all. It is this art which Husserl calls phenomenology.

In America phenomenology is not very popular. Many scholars in this country feel safe when they use the established concepts and methods of science. They do not like the idea

that, in philosophy at least, such procedures should be postponed until, in a patient survey of the thing in question, its properties have become thoroughly known. For this attitude two rather plausible reasons may be given. *One* of them is that phenomenology seems dangerously similar to Introspection, which is generally regarded as a failure. The *second* reason consists in Husserl's very radical procedure when making use of the phenomenological principle. What indeed are "the things" as such to which we should go back? What in our view of these things, logical and otherwise, has to be regarded as a foreign ingredient, brought in by naturalistic habits of thought? According to Husserl even any conviction about existence and occurrence in a chain of facts has to be discarded in our initial attempt towards a purely phenomenological treatment of material.

I do not think that the first argument is altogether justified. The virtual phobia which has developed with respect to the "Introspectionist" treatment of immediate experience hardly befits the scientific mind. It seems, however, as though there might be some force in the second argument. In trying to discard all hypothetical elements from our view of things we may easily and inadvertently be influenced precisely by hypotheses about this procedure. What constitutes the truly phenomenological nature of something? What is to be regarded as an hypothetical addition to it? What, in other words, is the criterion which Husserl applies when he separates one from the other? I am not sure whether Husserl is fully aware of the danger latent in these inquiries. The main objection against Introspection was not its interest in "things themselves" but rather its arbitrary selection of some aspects of experience as genuine and others as artifacts.[6] If Husserl were in danger of proceeding in a similar way, then a similar criticism would of course apply to his kind of phenomenology.

I shall now try to state Husserl's main ideas about logic in a slightly simplified form.[7]

[6] *Cf.* the writer's *Gestalt Psychology*, 1929, ch. 3

[7] I hope simplification does not make my report misleading.

His first thesis is that logical requiredness has little to do with psychology, not even with the psychology of thought-processes. Psychology is a science of facts as they really happen. So conceived, it is full of those complicated and doubtful naturalistic premises which we have just mentioned. Besides, however, since psychology is a science of facts there is, in this field, no adequate place for the distinction between "true" and "wrong" or "untrue." Facts are never wrong or untrue, nor are they ever true either. They are or they will occur, nothing else. "To be true," on the other hand, is a special case of intrinsic requiredness. It seems evident, therefore, that truth has to be sought and found somewhere outside the realm of mere facts, more particularly outside the field of psychology.

We have to add, furthermore, that a solution in the Kantian style does not agree with the principles of phenomenology. What, after all, is the character of Kant's statement that all requiredness is a consequence of the structure of mind and of its forming power with regard to the "material"? The statement is a *hypothesis,* since nobody has ever observed how, by this power, an altogether indifferent material was given form and requiredness. Hypotheses, however, belong in the world of facts, of science, where all our troubles of relativation have come from. Indeed, it is difficult to decide whether Kant's own alleged salvation of requiredness does not also yield relativistic consequences.[8] Kant was led to his theory by the conviction that the material given to us cannot possibly arrive

[8] Not a few philosophers and scientists really did interpret Kant as though his epistemology meant a special form of subjectivism and relativism. If the "structure of the mind" is regarded as a definite fact which actually is as it is, but which might be different quite as well,—then we are as near relativism as possible.

in our mind with its order and form given at the same time. But he never even tried to prove this point. Husserl, with Stumpf and Dilthey, declines to accept Kant's assumption: according to him the material, let us say of perception, is given with much of its order and form directly. One more reason, evidently, for going "back to the things themselves." [9]

I have to add at once that, for Husserl, all insight, i. e., in this connection all awareness of logical requiredness, is strikingly analogous to perception in the usual meaning of the word. When realizing the truth of a statement we do not look upon any operation of our mind: we look rather upon the content of the statement as upon something outside, distinct from our mental operations. From the phenomenological point of view, then, and in this sense, there is no "subjectivity" in the logical requiredness of a statement, since the logically required is before us, as an object is before us when we perceive it. It is not only before us, however. When looking upon it, we are, according to Husserl, even looking beyond the world of facts. If this seems strange at first, it becomes more plausible if we consider an example. In elementary geometry much use is made of the principle: Things equal to the same thing are equal to each other. When considering such a principle our situation is not essentially different from that of perception. Indeed our attitude is strictly similar to that of looking carefully upon an object. Besides, however, the object in question has noteworthy properties. The words, for instance, in which I clothed the statement represent definite concepts. Well, then, is the content of a concept like "equal" before us as something which merely

[9] In more recent years Husserl's phenomenology has taken a turn which brings it nearer to Kant's epistemology.

happens at this moment? Not at all. It is looked upon as an entity which is always ready for our inspection but does not depend upon it. It remains the same in a world of concepts in general, whether we consider it or not. When we inspect concepts, therefore, we somehow seem to look beyond the world of mere facts, psychological facts included. All of these have their definite and particular times of occurrence, all have their definite duration, whereas the content of a concept seems to be outside the course of events and of time. The term "duration" or any date does not apply to it just as the term "rectangular" does not apply to "despair." What is true of our terms inasmuch as they represent concepts remains equally true for the principle as a whole. It is a correct principle. But is its correctness something that happens or exists this very moment, that may happen occasionally, while at other times it does not exist? Even to speak about it in such a manner seems inadequate. When realizing it we look, in this sense, beyond the course of time and beyond the facts which may occur in time. Truth and correctness are in a timeless ideal world of their own.

If our example appears too abstract to be convincing, take another. So far as we are not color-blind all of us know what is meant by the words "red," "blue" and "purple." Here is a statement involving all of these terms: Purple is a visual quality which as such has its place between the red and the blue. The statement is true. Considering merely the degrees of similarity which prevail between the three qualities we cannot arrange them otherwise. The order is "required." Does this mean that, at this very moment only, our concept of purple has a place between the red and the blue? By no means. We see immediately that, quite independent of time, the statement

corresponds to the relation before us. Its content and its requiredness are timeless, we might say eternal; it remains the same and true whether we look upon it or not.

This remarkable property of genuine truth is to be compared with the behavior of facts. Facts are not only in time and therefore perishable; from the standpoint of knowledge they have another weakness. When once we have observed a combination of facts we never know exactly when, or even whether, the same combination will occur again. Natural science, it is true, has its so-called "laws." According to them a fact, observed carefully a few times under definite conditions, will happen again as soon as those definite conditions are given once more. The principle underlying all such laws, the principle of induction, may well be applied everywhere in empirical science and may even be indispensable there. But nobody has yet been able to give a real proof of its soundness, nor has its use in the handling of facts and the stating of laws ever been finally justified. The reason is apparently that no facts are *evidently* generated by other facts, that there does not seem to be any evidence of necessary sequence. Laws of fact can only be accepted, therefore, so long as they are verified. And no amount of verification can possibly make them more evident. At least we never know definitely whether we have recognized all the conditions involved. Consequently we never know whether such a law has been formulated with perfect accuracy. Let us consider once more "evident" truth as revealed in phenomenology. Such truth has no degrees of accuracy, and it does not depend on "verification." Once we have looked upon it with our mental eye, the relation of the colors red, blue and purple is determined completely and finally. It follows that truth,

requiredness in thought and the laws of logic can never be successfully interpreted as the outcome of "laws" about psychological or any other facts. In the two cases the term "law" seems to mean two different things.

This is Husserl's solution of the problem in the case of logic. Man is able to look into a world of timeless entities and to read off from them evident truth, just as in perception he is reading off matters of perceptual fact. This last escape, therefore, in which even the world of psychological facts is left behind, seems to lead into a strange country where truth is safe. And truth, I repeat, is a case of intrinsic requiredness.

Thoroughly fact-minded people will be inclined to deride Husserl's modernized Platonism. They should not do it, however, before they have tried to give an adequate theory of immediate evidence as exemplified, e.g., in the case of those three colors. If phenomenology is to be criticized, much may also be said in favor of its description of logical evidence.

Once more, it is true, we find intrinsic requiredness located in some particular realm, and once more we find this realm adorned with dangerous words. However, the location as well as the application of such attributes are, in this instance, based on a certain type of observation, curious though it may appear to us. There is, phenomenally, something remarkably "objective" about conceptual truth in so far as its content seems segregated from the stream of thinking. There is, besides, the remarkable difference between the directness of "evidence" or requiredness on one side and the indirectness of guesses and of verifications of factual laws on the other.

Nevertheless I cannot admit that, with Husserl's description, analysis of intrinsic requiredness has been completed;

even less that the whole field of such requiredness has thus been covered, and least of all that this field is altogether outside the world of facts.

May I begin with the last point. If the world of truth did not extend into the world of experiences—and that is: of facts—how should we ever know of it? Inspection of truth, as Husserl might call it, is an actual fact. As such, the inspection could hardly reach its object, if the object did not belong to the same realm of facts. Facts and the material of evident truth seem after all to dwell in a common universe. Concepts and evident truth, moreover, may bear phenomenally the character of timelessness, of independence with regard to our reading and looking. But why not? In common perception all solid things bear, phenomenally, the character of being indifferent to and practically independent of my looking upon them. There is something of permanency in their appearance, beyond actual perception. Yet nobody would deny that these objects belong to the world of facts. Permanency, it is true, is something different from timelessness. But the example shows at least that, phenomenally, even objects of simple perception do not necessarily share those temporal traits which we ascribe to perceiving as an event. Not all "facts" seem to behave in the same manner.

I shall now return to the second point. Banishing all requiredness into a world by itself Husserl has evidently pictured the world of facts as much poorer than it really is. When something appears as required to people's eyes, they do not only and not always look into a world outside their lives. On the contrary, nothing can be more obvious than that, over and over again, they do definite things and they abstain from others because those real things are actually required and these are not. According to Husserl

mathematics is a system of truth outside the world of facts. But an engineer who has to build a bridge will construct the bridge as mathematically required, and thus truth will become immediately effective in the middle of a world of facts. Mathematical truth can be transmitted from one man to another at a certain date and in a definite place. Truth has often economized human labor and physical energy. There is no doubt then that truth may at least in some cases be factual. Husserl is aware of this though he might perhaps object to my formulation. But I do not see that he gives a real explanation of this amphibian nature of truth. Amphibian it is: Give a clever boy the premises necessary for a sound conclusion, and he will soon experience how, in his actual thinking, the conclusion is born as a real fact, how it becomes required in his stream of thought at a definite time. In this manner mental life at least is full of dramatizations of just such truth and requiredness as Husserl prefers to consider in its detached givenness only. I do not deny that a special interest may be found in the timeless variety. But even that, we found, is not altogether outside the realm of facts. Once more, we seem to find an ambiguity in the meaning of this term "facts." Should we, in careless speech, give it a much too narrow connotation? With a puzzling contrast between "facts" and "requiredness" we have begun our survey of the situation. Now, it appears, requiredness may occur at least as a psychological reality since it operates in mental life and has, even beyond it, many effects which everybody would call facts.

III

What could a *psychologist* propose as an elucidation of requiredness? Some opinions which one hears occasionally expressed deserve our critical attention, although few philosophers would fail to recognize their weakness.

Whatever the nature of values may be, if for the moment we do not include logical requiredness in the meaning of the term, we have to admit that from person to person, from tribe to tribe and from historical period to historical period accepted values seem to vary enormously. It is this fact which makes some influential theorists deny that a system of ethics can be built on a basis acceptable to all people. At least the axioms of the system, they would say, could be arbitrarily chosen. Nevertheless the phenomenon of value as such remains, whether or not there is agreement about definite values. And precisely the variability of values would make some inclined to believe that value has to be interpreted in terms of *habit.*

Apparently such a theory would have a great advantage. There is no word which sounds as bad to most psychologists as "purpose." Its meaning is supposed to contradict the very principles of science. And "requiredness" seems to have a flavor of "purpose" in its connotation. An attempt might therefore be made to reduce requiredness to terms which, from the standpoint of sober science, would be more acceptable. Perhaps this might be achieved, if habit were the essential fact underlying value.

Do *all* habits contain a factor that would make us understand value? I have, for instance, the habit of winding my watch in the morning. So thoroughly is this a habit that I am for the most part hardly aware of my activity. Is there any requiredness in the habit as such? One might

say: That the watch be wound is required in view of its practical use. But this statement does not contain the term habit or any similar concept. Thus, it would seem possible to speak about value in a context which does not involve the concept of habit at all. This would clearly create difficulties for a "habit theory" of value. It might be said, however, that under these circumstances, considering the need for mental economy, an automatic procedure would meet the situation much better than a fully planned action every morning. This statement is full of valuation, but this valuation is certainly not made clear by the concept of habit as such. On the contrary, the value of the habit is made dependent upon the fact that, in this case, habit fulfills certain conditions given in the general situation and—in other values. The implication is, most obviously, that in some cases a habit may fulfill such conditions, but that in other cases something different from habit gives value to an act. Consequently, as yet we have found merely that habit is one among other things which may appear as required. Thus the meaning of requiredness remains, in principle, quite different from the meaning of mere habit as such. The first is not claiified by the second.

Another example may bring us nearer to a "habit-theory" of requiredness. I have "the habit" of smoking; cigarettes and so on have a value for me. When I was six years old there was for me no such value in tobacco. The habit has developed and, at the same time, tobacco has become valuable for me. Whoever is sensitive to nuances in the meanings of words may feel at once that, now, "habit" has a slightly different connotation. What are the facts? There was a development, some form of "learning." But it was learning of a particular kind: Some time ago

I had not begun to "like" something; now I "like" it. What makes smoking a value for me? That it was developed in some form of learning process? Or that there is a liking? Let us suppose that the same liking existed without the previous learning. Would tobacco and smoking then be a value for me? They would. Consequently not the process of habituation but the liking is the essential side of this particular value-situation. This becomes even more obvious if we change conditions a little. In a fit of dizziness produced by some intestinal trouble I am offered a cigar. I refuse with horror to accept it. Even the smoking of others is detestable at the moment. This reaction is one of negative valuation, and it has nothing to do with habit except in so far as it indicates that even a strong habit may be totally overcome by a direct dislike.

One might object that, after all, habit-formation has in our example produced the liking and the value. But, even supposing that this were true, liking as such would still not be definable by its previous history. It would remain liking, and as such it would be responsible for the value wherever it came from. And what is the real history of the case? The first experience of smoking, usually when it occurs in early years, has several effects, some of which are disagreeable while others, perhaps barely perceptible, are rather pleasant and are liked. I remember perfectly how I discovered these as a boy. Some physiological process, also observable in the case of other poisons taken in small doses, makes the unpleasant components disappear rapidly. At the same time, naturally, the others soon become conspicuous and are correspondingly better liked. At last only the liking is left, and it becomes firmly established. That it is thus established and automatized is the habit-side of it. But that it constitutes value at all is due

to its character as liking.—It comes then to this: What we call habits in this and similar instances is another expression for liking, automatized in its occurrence and consequences by habituation. In the liking there really seems to be something closely related to value. Without the liking, in any case, habit would be a mere matter-of-fact connection between certain situations, objects or ideas and certain responses. Requiredness would not come in at all.

Extremists might object that in this argument I am presupposing some distinguishing meaning of "requiredness" which, according to their view, can never be found because there is no such thing. Somehow our primitive ancestors, lacking as they were in scientific training, began to use such fantastic terms. But these have no real meaning which could not be translated in terms of mere facts. We should not bother about superstitions.—My answer would be that these statements themselves are regarded as "valid" by those who make them, that "should not" means a case of negative requiredness if we understand each other at all. Moreover, I would say, it is almost inconceivable that man should, for what must be now thousands of years, have distinguished between a *quaestio juris* and a *quaestio facti,* that in our times people should discuss the strange difference between mere facts and cases of requiredness without any basis for such a distinction in their experience. Such an assumption seems rather bold from the standpoint of causation, so that, as a scientist, I am loath to accept it.

It may not be altogether superfluous to add one more remark. We sometimes say that when a reaction has become *associated* with a given situation, the animal or man in question "tends to" react in that manner whenever he finds himself in the situation. Obviously this is no elucidation of our problem from the standpoint of an associationist. *Either* the term "tending to" is used here as an equivalent to "being inclined to" or even "wishing." In this case quite a new factor is introduced which, whether brought in by an associationist or by somebody else, has no essential connection with the

concept of "association" as such. *Or else,* it is a case of mere figurative speaking,—as when the meteorologist would say that, in our latitude, high pressure "tends to" be accompanied by frost in winter-time. If this is the meaning, it expresses no more than a probability of factual occurrence, and has nothing whatsoever to do with requiredness.

If philosophers should think that all this discussion is far too primitive, that no scientist could possibly have ideas such as those considered in these last paragraphs, I envy them their optimism. Strange things have often been said about such problems ever since requiredness became unpopular in science and since, consequently, trained thinking about it grew less common. It is fair, however, to admit that in recent years such notions have begun to lose influence.

As a second explanation it might be said that value is a term equivalent to "useful" or "expedient." Such terms refer to human activities, and thus requiredness would become a correlate of these. It is generally recognized, however, that nothing is useful or expedient on its own account. Both terms point to something beyond, which is the thing primarily required. What is useful or expedient owes this character to another and more immediate value. And since it would be futile to explain the simple case in terms of a complication we have not made any progress. This is, by the way, one reason why so many philosophers are not satisfied by a Pragmatistic theory of truth. If the meaning of "truth" is reduced to that of "expedient," our gaze shifts to a greater distance in search of a point at which abstract expediency might become concretely expedient. But until a definite theory of value is added we can find no stable point on which to focus.

One might try other terms such as "pleasant" and its negative counterpart "unpleasant." If, however, the last attempt failed because primary requiredness cannot be understood in terms of value that is indirect and bor-

rowed, this further interpretation fails because a very special example is not a theory of the general concept. I know the meaning of such symbols as 3657 and 19. Asked for the sum of these numbers I should answer 3676 with perfect assurance that this is strictly correct and required. At the same time I am not aware of any pleasantness. If there is any it is not worth mentioning. Pleasantness, therefore, does not accompany requiredness in such a manner and so necessarily that we could use it as an interpreting notion. If it has anything to do with requiredness it is at best a very special case by itself,—no less so, by the way, in ethics and aesthetics than in logic. If we should try to apply the term more generally, its meaning would soon be changed beyond recognition.

Quite apart from this criticism, the name of some quality which certain things may possess does not as such explain how requiredness is related to mere facts. This is true whatever adjective or noun might be chosen instead of pleasantness. One may assume, however, that implicitly the term has some further connotation because of which it is mentioned in this connection. Such a hidden meaning becomes indeed more apparent if pleasantness is translated as "attractiveness." In fact, if a *dynamic* trait is inherent in those things which are pleasant or unpleasant then we seem to have made some progress. More particularly such a trait might be attributed to either of two entities, the *object* in question which would attract or repel the subject,—or the *subject* which would strive toward or away from the object. The first case would represent a remarkable phenomenon. According to a wide-spread opinion the objective side of our experience is strictly passive and can only *appear* as active, if some "pathetic fallacy" is involved. We shall come back to this problem. For the

moment it will be advisable to search for the interpretation of value on less debated ground. If, on the other hand, the meaning should be that we strive towards certain things which are called pleasant and turn away from others called unpleasant, then we are very near a subjectivistic theory of value which demands immediate attention. The outline of it became first visible when the "habit-theory" of value was discussed. It should be clear, however, that even with this dynamic interpretation such a term as "pleasant" would still point to a very special example. Not pleasantness but striving would have to be the essential condition of all those instances in which requiredness occurs; for we strive toward many things, and only a few of them may properly be called pleasant.

Strictly negativistic theories, the authors of which refuse to acknowledge requiredness as one essential side of our experience, should be clearly distinguished from other explanations in which "directed attitudes" are recognized expressly as a central part of mental life and taken to be the basis of requiredness People like or dislike things, they seek some and avoid others. To the contrast between "ought to be" and "ought not to be" there corresponds the antithesis of such verbs as point to opposite tendencies in human striving. All striving has an objective counterpart. According to the nature of the attitude this counterpart is called a demanded or a rejected thing. Striving is perhaps too special a term for all those directed attitudes which make a thing required or the opposite. Professor Perry, the outstanding representative of this theory among the moderns, takes "interest" as a term the meaning of which might easily become sufficiently general to include all special cases. Whatever the term, however, we are here obviously on firmer ground. Something "ought to be," it

"should be" from *my* point of view, it is "asked for" by *me*. This context seems at first sight to make the meaning of requiredness perfectly understandable. Mental life is "hormic," as Professor McDougall would call it. Probably for this reason the theory has appealed to several philosophers. It seems so simple and natural. It was boldly defended by Spinoza when he said: "In no case do we strive for, wish for, long for or desire anything because we deem it to be good, but on the other hand we deem a thing to be good, because we strive for it, wish for it, long for it, or desire it." [10] The same view is held by Hobbes. In our time, von Ehrenfels seems to adopt the same theory, though, as Professor Perry remarks, in his case as in others there is a tendency to confuse the generic problem of value with that of a hierarchy of particular values. From Professor Perry we have the statements: "That which is an object of interest is *eo ipso* invested with value," and also "an object is valuable when qualified by an act of interest," i.e., in the experience of value the relation of an object to interest "assumes the rôle of adjective." In a simple formula: "x is valuable = interest is taken in x." [11]

This view again has often been criticized. On the other hand, there is something in it which makes us feel that at last we are in direct contact with our subject-matter, that we are not trying any more to put something else in its place. Just how the introduction of interest or striving could produce this feeling of relief, why the contrast between mere facts and requiredness seems less disturbing now, these are questions which we shall attempt to answer next.

[10] *Ethics*, Part III, Prop 9 (Transl by R H M Elwes, 1901)

[11] *General Theory of Value*, p 115 f W. McDougall, I suppose, would approve of Perry's statements

CHAPTER III

AN ANALYSIS OF REQUIREDNESS

I

IN Professor Perry's theory of value the essential concept of interest seems intimately connected with biological considerations. Interest, he says, is to be investigated "upon the plane and in the context of physical nature," because the limitations of introspection have been "most flagrant . . . in that department of human nature with which theory of value is primarily concerned." When trying to define "interest" we have to use terms like "conation," "tendency," "striving." If again their meaning is to be clarified we naturally and unavoidably fall back upon "the action of the physical organism." [1] In this point I cannot follow Professor Perry. One reason is that here we have before us one of the major and most difficult *problems* of natural philosophy. Assuming that his theory really moves on biological ground, is it advisable thus to burden the foundation of a general theory of value with assumptions about interest as a property of physical systems? I do not think so. Precisely because such hypotheses will be discussed later I shall try to keep the treatment of requiredness as such independent of them. A second reason is that one may doubt whether Professor Perry's theory really uses biological concepts, whether, as a matter of fact, his arguments do not belong in another sphere.

[1] *General Theory of Value*, pp. 141-144.

It may be essential for philosophical method that we see our way clear with respect to this.

Animals, so Professor Perry says in explaining his biological theory, perform adaptive acts "in the *expectation* of . . . consequences." "Owing to the capacity of memory, life is circumspect and *prophetic*." The animal "acts not because of what is or has been, merely, but because of what he *anticipates*." His attitude is one of fear or hope "of something, which something is not upon the plane of past or present physical existence as ordinarily conceived." [2]

I agree with Professor Perry when he holds the view that psychology should proceed in conjunction with biological research. I doubt, however, whether at the present time there is any strictly biological evidence for his statements as just reported. Let us take an example: All valuable accounts of conation "appear to incorporate something of the action of the physical organism." [3] This statement may refer to the body of a subject who is trying to characterize his own conation or, also, to the case in which the behavior of others is characterized by conation. I assume that all biological statements refer to the organs of certain physical systems, the organisms, and to the physical operations of these organs, even if the organism "as a whole" is functioning. If this is accepted, no strictly biological observation has, as yet, discovered anything like conation. Biological science as such has only found what, under given conditions, actually happens in several parts of the organism. Muscles contract, action potentials travel along nerve paths, glands secrete; but in biological observation as such there never occurs any tendency or any

[2] *Ibid.*, pp. 177-178.
[3] *Ibid.*, p. 144.

striving, nothing prophetic and no anticipation, quite as little, indeed, as in any observation of the physicist.

It might be objected that in action we have, for instance, a feeling of "effort" in our muscles. But this is an ambiguous statement. We do not feel efforts in our muscles as physical organs. Just as a star as an object of astronomical research is not a visual object, just as in this case the visual object has to be clearly distinguished from the astronomical object, so the phenomenal body as a percept is to be distinguished from the physical organism. The phenomenal body has parts, e. g., arms and legs, which are given visually and otherwise; in action it undergoes a great many changes, such as those "efforts" which are localized in (phenomenal) arms, legs and so on. We are aware of these phenomena, and perhaps much of conation may be characterized as definite states in such parts of the (phenomenal) bodily self. It is clear, however, that in this case we are not making use of biological evidence. We are rather dealing with purely phenomenal data.

Again it might be objected that very often animals "look" their expectation, their conation, striving and so on. This is doubtless true as a description of those percepts which we call higher animals.[4] As soon, however, as we apply to the corresponding physical objects the methods of natural science, e. g., of biology, nothing of these aspects is left. We still find that the physical systems in question move in the presence of certain environmental objects. But if we should say that they move "in order" to reach such objects or to get away from them, that they anticipate future events, our expressions would have left the field of evidence to which pure biology is as yet confined. It would be an *interpretation,* and certainly not an interpre-

[4] *Cf* the writer's *Gestalt Psychology,* ch. 7.

tation on the basis of natural science. Why in an animal as a percept, i. e., in the phenomenal realm, there should be conation and striving, whereas the methods of natural science when applied to the corresponding organism do not yield anything of the kind, is a moot question. But we have to admit the difference as it is. It may be plausible that, if we could share the animal's immediate experience, we should find in it striving and conation, however localized in his phenomenal world; but this is not a thing biologists are able to do. If they were, they would make simple phenomenological statements instead of those observations which belong to natural science. One cannot argue that the term "biology" should not be too narrowly conceived, that biology, too, could make use of all varieties of percepts and phenomena. This is not the point we are discussing. Our question is whether, in characterizing conation or interest, we can achieve more by adding *specifically* biological evidence to psychological observation in all its forms and made by any kind of people. The answer is: No; because biology proper keeps "upon the plane and in the context of physical nature," and because in this context no data of conation have ever been found.[5]

The shortcomings of technical introspection in psy-

[5] In this discussion I have distinguished between percepts as parts of the phenomenal world and physical realities in the physical environment which correspond to them. I know that in some forms of modern Realism this distinction is not accepted In our connection, however, the point cannot be raised We are asked to apply the concepts of biology. And if there is overwhelming evidence for anything in biology, it is precisely this: that percepts are based upon processes inside the organism. How then are they to be identified with parts of the physical environment? We do not identify the sun with a chemical reaction which its rays produce in some sensitive chemical material after travelling eight minutes through astronomical space. According to biology the percept "sun" is functionally no less distant from the physical sun than is the chemical reaction. (*Cf.* also ch. 4, pp. 109 ff)

chology have made Professor Perry reluctant to apply this method in his characterization of interest. I sympathize with his attitude in so far as artificial and prejudiced analysis of immediate phenomena is concerned. But why should simple and unsophisticated—I might say: macroscopic—awareness of conation and striving be condemned? It cannot be futile and worthless; because, without any exception, whatever Professor Perry himself brings forward in his excellent characterization of such concepts is based on precisely such immediate awareness taken naturally and at its face value. Contrary to his belief, what we call biology could not have given him any help in his endeavor, and what he did achieve was based on sound description of phenomenal facts. Biological observations in the customary sense did not contribute to his psychological analysis; but perhaps biology will in the future profit by his phenomenological insight.

Under these circumstances it will be advisable to do frankly and explicitly, even against the fashion of the day, what we must do perforce. Biology with its explanations will come into the picture later on. But the theory of value in general, of value generically, has to use the phenomenological method. There is, in fact, no other method since requiredness or interest does not as yet occur among the data of any science of nature. All these data seem foreign to our subject matter. If we were to attempt to make use of them at the outset we should speedily find ourselves lost.

At the present time the ideas of modern Positivism are beginning to unite with those of Behaviorism. In this process Behaviorism seems to gain new force. Phenomenal data, so the Positivist tells us, are evidently the primary material from which all scientific enterprise starts. But by no means, he

adds, is the interest of the scientist concentrated upon them. We live among them, we know them, and poets—who, with philosophers, seem to be regarded as an inferior branch of mankind—dwell permanently in their midst. But science does not wish to experience and to know, it wishes to explain. And explanation is the building of constructs. There we are· Phenomenal data do not occupy real science. Behaviorism was right after all.

It may be granted at once that the final aim of science, including psychology, is explanation. It cannot be admitted, however, that for this reason psychologists should not be interested in phenomenal data. It seems natural to acquire at least *some* knowledge of those data which we intend to explain, which our constructs are expected to fit, before we begin the construction. Otherwise, why should the constructs fit? And it is not true that we know those data sufficiently. If I were a Positivist I should, for this reason, insist upon phenomenology as the genuine basis of all explanatory construction.

This statement, however, has to be qualified: For our purposes phenomenology is not to be restricted to the realm of logic and of timeless entities. If, furthermore, some exponents of phenomenology tend, by their work, to make us believe that their occupation is necessarily sterile, this is their fault, not that of the procedure as such. Even more important, the very vaguest speculation has sometimes found a shelter under the roof of phenomenology. With such aberrations we cannot wish to have any connection.

Phenomenological description is still apt to be misunderstood almost at every step. One reason for it is that in phenomenology, if it does not wish to introduce a completely new language, many terms have to be used which, in explanatory science, are often employed in a different sense. In order to avoid such wrong interpretations I shall make a few statements about the phenomenal field in which the connotation of the most necessary terms will

become apparent. I do it as the risk of repeating what has been said before.[6]

Phenomenally, the bodily 'self' [7] is not a physical entity outside immediate experience as is the physical organism; it is, rather, a percept of which we are aware, enriched by changing moods, attitudes, efforts and activities. Similarly 'objects' in this discipline are phenomenal things, for instance percepts. (Images, concepts, and the like, are also called 'objects,' and not without justification.) Percepts are, of course, not ghosts belonging somehow to the phenomenal 'self.' Those, at least, which we call 'things' look, in general, most 'real,' 'independent,' 'permanent' and 'substantial'; they often feel 'heavy' and as a rule 'resistant.' All this implies in no sense a contradiction of their nature as phenomenal things. Their place is not in the 'self'—why should these percepts be localized inside another particular percept?—, but in other parts of phenomenal 'space,' near or far as the case may be. In this world which is that of naive everyday life, certain parts, events and properties belong—phenomenally—to the 'self,' others belong to 'objects' or, more generally speaking, to the phenomenal environment of the 'self.' The former have the character of 'subjectivity' which, in this sense, is only another name for the fact that they appear and are counted as parts or states of the 'self'; the latter have, in most cases, the character of 'objectivity.'

It is most essential for phenomenological statements that

[6] *Cf Gestalt Psychology*, ch. 1 and 7

[7] The ambiguity of such terms as self, objective, subjective, real, seems to me so dangerous that in the next paragraphs I shall mention them as 'self,' etc. whenever they refer to phenomenal data. To continue the same procedure throughout would be too cumbersome I shall, however, return to it where a clear distinction between the phenomenal and other meanings of concepts is particularly difficult and at the same time most important.

they never be confused with hypotheses or even with knowledge about the functional genesis of phenomenal data.[8] Where a thing has come from, to what its existence or that of its properties is due, is a valid question, but for the most part not a question for phenomenology. What properties the thing actually has—this is the question of phenomenology. Personally—but this goes beyond phenomenology—I share the opinion of those who contend that all phenomena without any exception are the correlates of somatic processes in the nervous system. To this extent they are, all of them, *genetically subjective*, whether phenomenally they have the character of 'subjectivity' or whatever their degree of phenomenal 'objectivity' may be. In this other sense, however, subjectivity means dependence on the *physical organism* and its functions; it does not mean dependence on the phenomenal 'self' or belonging to this 'self.' In our present terminology the 'self' is, at any moment, one special complex of phenomena, surrounded by others. Genetically or functionally it depends upon a special complex of processes in the brain.[9]

It is quite as important to realize that the meaning of 'objective' in the phenomenal field has no direct connection with physical existence outside the physical organism. We tend to call such properties of our phenomenal environment objective as have a counterpart in the physical environment of the organism. But the green color of a leaf is phenomenally 'objective,' even if we decide that no colors whatever occur in the physical world. For black

[8] To be sure, the inductive procedure of science with all its hypotheses and constructions *is* a subject-matter of phenomenology. This does not mean, however, that the *content* of such assumptions should be mixed with purely phenomenological propositions.

[9] *Cf* ch. 4, p 129

there is not even a direct stimulus, and still it is phenomenally 'objective' as the property of certain surfaces. So far as we know there are no particular connecting physical forces between just those stars which, as a phenomenal group, are called the Dipper. Still, this group-unit as segregated from other stars is 'objectively' there in the phenomenal 'sky.'

It is even more essential while more difficult and less customary to discard in phenomenology all notions of direct or indirect origin from those of 'subjectivity' and 'objectivity.' It may be that we *learn* to localize in 'objects' certain properties which, originally, were not inherent in them. Perhaps in some such cases the origin of those properties has been the 'self' with its wealth of traits and events. If now, in adult life, such properties are localized in 'objects' and share their phenomenal 'objectivity,' this is a phenomenal fact which is not altered in the least by the theory or perhaps even the full knowledge that previously they were not there at all. Possibly in earliest infancy the boundary between an 'objective' and a 'subjective' part of the field was, phenomenally, quite different from what it is in adult phenomenology. This, however, does not change the boundary as it now is. *No explanation can change a phenomenon or its location.*[10]

[10] It is not yet customary to distinguish clearly among the different meanings of such terms as objective, subjective, self, etc. Under these circumstances it seemed to me advisable to draw in this chapter a sharp dividing line between concepts which refer to the physical world and others which apply in the phenomenal realm In this respect, I believe, "phenomena of transcendence" lead to a certain complication A few of them will be discussed in other chapters. If I were to do it here the reader might easily become confused.

II

I return to our main problem, which is the generic problem of value as such. The subjectivistic theory of requiredness seems to resolve the paradoxical aspect of this notion. Apparently the mere introduction of human interest, striving, conation achieves the solution of the problem. We have the impression moreover that the problem is, in this way, not merely solved, but rather transformed into something obvious; because striving and interest themselves are matters of everyday experience. It is precisely this aspect of the theory which may arouse one's caution. Too frequently just the apparently obvious contains, and most successfully hides, certain essential traits which deserve all our attention. I shall try to show that this applies to the present case.

What was paradoxical in requiredness? It appeared paradoxical so long as we said: Facts *are* or *happen indifferently*. There is no requiredness about them. Consequently there is no place for requiredness in a world of facts.

How, then, is this situation changed when requiredness is brought into connection with interest? The subjectivistic theory makes us see that we were too hasty in our characterization of *facts*. Not all of them are or occur indifferently. In the very nature of some facts there is, as a constitutional trait, a quality of acceptance or rejection of something beyond. Human interest, striving, conation are all of this kind. It belongs to their character that they point or refer to other facts. And this reference to other facts is far from neutral. They are very partial, they are selective with regard to other facts to which they refer. As soon as we make these properties of interest more ex-

plicit all apparent commonplaceness disappears from the subjectivistic theory of requiredness.

The first point, it is true, is still simple enough and not a novelty; some contents of the phenomenal field have a *direction* or directedness, others not. A coin before me does not point toward something, an interest does. Because of this property we shall borrow a term from mathematics and physics and call interest a *vector*.[11]

With the second point we approach an essential side of our problem which is usually well hidden under the disguise of obviousness and commonplace speech. Interest as a vector is experienced as *issuing from a definite part of the field*. If it is 'my' interest, it issues from that particular item in the field which I call 'myself'—not from a pencil to the left, not from a sheet of paper to the right. Why repeat what everybody knows and what our language implies? We do so because we are dealing with one of those cases in which experience does contain not merely an isolated fact here and an isolated fact there, but also the fact of their *belonging together*. This is the phenomenal aspect which practically is more implicit than explicit when we say: "I am interested in," or when we speak of "my interest." There are other such experiences of belonging together, many of which have been dealt with by Gestalt psychologists.[12] This particular one, however, in which a vector is experienced as issuing from a definite part of the field has a special relevance in our present discussion.

[11] K. Lewin and his students frequently use the term in their investigations.

[12] *Cf.* Wertheimer's article in *Psychol Forsch* *4*, 1923, the writer's report in *Psychologies of 1925* and *Psychologies of 1930* (ed by Murchison), *Gestalt Psychology*, ch 5 and 6, and Koffka's *Principles of Gestalt Psychology* 1935.

A third point is no less implied in common language than the second, but is in the same manner hidden by the smooth cloak of everyday speech rather than really accentuated. The subjectivistic theory of requiredness often refers to objects as *causing* or *releasing* a human interest. This is one aspect of the rôle which objects of all kinds play in subjective valuation. Another aspect is that interest or striving is *directed toward* the phenomenal object in question. Not all causes have such effects. But it is this effect in our case which, implicit in common speech, has to be made explicit. Interest is not only experienced as issuing from a particular part of the phenomenal field. It is also experienced as referring to another and, in most cases, a very definite part of the same field. Here, then, we have a vector which, with two parts of the field, forms an experiential unit, a specific context.[13] The three belong together in experience; one part is the point of issue of the vector, the vector transcends into the objective region of the field, and the last part serves as target or mark for the vector. In this case at least Hume's bundle-description of the phenomenal field is utterly inadequate, because definite organization is here a concrete trait in the field itself. There is in actual experience no more doubt about the point toward which interest is directed than about the point from which it issues.[14] Innumerable times in philosophy and psychology some such expression has been used as: "something is the object of an interest." But few seem to realize that the full meaning of such simple terms is sufficient for a refutation of

[13] I know of no English word that would correctly render the meaning of the German "Zusammenhang." In this predicament I have decided to use the word "context" as a substitute For the purpose of this book it will perhaps acquire the connotation which is implied in the text

[14] *Cf Gestalt Psychology*, ch. 10

Hume's atomistic psychology. We *are* aware of definite and very concretely organized dynamic contexts. There are not separately: a self, an interest and many things in the field, but, surrounded by many other items, a-self-interested-in-one-definite-thing.

Under these circumstances it is not very important whether we say that an interest is directed from the self to the object or, perhaps better, that in the form of an interest the self is directed toward the object.

In such organization, as we all know, the vector of interest may be qualified in a great many different ways. It may have the quality of hatred, of fear, of contempt, of approval, of love, and so on All, however, have this in common, that by such vectors the self either accepts or rejects the corresponding objects.[15] It is this trait of interest-situations with its two possibilities which gives the subjectivistic theory of value its plausibility. It does not, however, make requiredness a commonplace matter. That one part of the field should be directly experienced as accepting or rejecting a definite other part of the field—thus formulated and deprived of the staleness of everyday speech—the statement contains a most remarkable fact.

Where all this leads to will soon become apparent when we discuss a last point. Professor Perry states that "any object acquires value when an interest is taken in it," or also "that which is an object of interest is *eo ipso* invested with value."[16] I do not see quite clearly whether or not a new property is thus attributed to the object when it becomes an object of interest. In general Professor Perry's

[15] For brevity's sake I use these terms in a general sense so that, for instance, in fear the negative character of this particular attitude would fall under the term rejection.

[16] *Ibid*, pp. 115, 116.

remarks point to the interpretation that he regards objectivistic terms like value and valuable as mere forms of speech the true meaning of which is not different from "interest is taken in something." [17]

We should not lose sight of the fact that some philosophers have never been satisfied by theories which localize all value in the self One reason for it may be that, besides those meanings which I have mentioned, the unfortunate word "objective" has still a third connotation in which it is nearly equivalent to "valid." Convinced that ethics should be a system of strictly *valid* rules these theorists would prefer an objectivistic interpretation of value since "objective" means "outside of us," "independent" and "valid" all at the same time. What is objective phenomenally exhibits, indeed, more steadiness on the average than does the everchanging stream of our subjective life. Besides there seems to be less variability among the objective fields of different people than among their subjective interests and tendencies I doubt, however, whether this is the only motive of those who insist upon an objectivistic theory of requiredness. Where in the history of philosophy one tendency of thought is never totally subdued, however excellent the arguments of the opponents, there is some suspicion that both parties look upon different sides of the phenomenological subject-matter, and that they are both right within limits. Even errors have often some basis in the phenomenal material, so that they

[17] This at least would follow from the thesis that "value is a specific relation into which things . . may enter with interested subjects" or that "relation to interest assumes the rôle of adjective" If a stone becomes warm when exposed to intense sunlight, its warmth is not, properly speaking, a relation between the sun and the stone The problem before us is whether the interest changes its object as the sunlight changes the properties of the stone (Cf also *General Theory of Value* pp. 28-34.)

are not totally wrong. In our case the objectivists are so insistent that it would not be prudent to ignore their claims altogether.

The same warning may be found in the fact that almost all naive people would be most indignant if we were to tell them that their interested attitudes contain all the values which they can find in the world, and that they are deceived when they believe that on the contrary objective values make them assume these attitudes. Charm is a special value-quality; so is loveliness and womanliness. Tell an unsophisticated young man who is very much in love that the object of the case has only neutral properties, and that to speak about her charm is just a synonym for the fact that he is in love. You will hear what he answers. Again, if you make the corresponding observation to a belligerent reactionary who declares that socialism and socialists are bad, he will emphatically refuse to accept the theory that without his hostile interest a socialist is a neutral object. No, he would say, these people themselves are bad. I may go farther and say that we find the same objectivistic conviction everywhere and exemplified in all possible varieties of value. This observation at least raises the question why, if the contrary is true, practically all mankind should not be able to see this simple truth, why they should hold precisely the opposite view, namely that the diverse forms of value are inherent in the objects.[18] It seems to me, by the way, that such apparent objectivity

[18] That the young man may be completely alone in his conviction about charm in his object is, of course, not to the point at all. Whether this concrete example of value is in his field a property of this object or not, is the only point we have to discuss here. And the same applies to the other example. Once more objectivity as here in question is not generality or general validity. Besides, I repeat, it does not mean physical existence either.

of values is of the very greatest practical importance. It would be ever so much easier to convince somebody that he is on the wrong track, if he could realize that value is equivalent to valuing, i. e., only an act of his own. But often he will be much too excited for such a conversion because the bad or the great, the mean or the noble, are so clearly before his eyes. And now you, his opponent, pretend that you cannot see what is so obviously there. How blind or stubborn you must be! Is not this our experience almost daily, for instance, in political discussion?

Personally I understand this objectivistic attitude of the layman very well because I find myself exactly in his position. That face looks mean—and I abhor it. Dignity I hear in those words which I have just heard Mr X. speaking—and I respect him. Her gait is clumsy—and I prefer to look away. Everywhere value-qualities are found residing in such objects as characteristics of them.

If this is true, there are, it seems, three possible interpretations: Just as objects are round or tall, events slow or sudden, so some have charm, some are ugly by themselves, independently. In this case the subjectivistic theory of value would appear to be at least incomplete. Again, if and in so far as interest of any kind is taken in an object, it *acquires* new concrete qualities, viz., value-qualities. This might mean an amplification or completion of the subjectivistic theory. And thirdly: Besides the self and its interests, other factors in a field could perhaps, also by a vectorial influence of some kind, create value-properties in certain objects. In this case, as in the first, the subjectivistic theory of value would be revealed as one-sided.

As to the first possibility I do not see any reason why such "tertiary qualities" should not occur on the objective side of the phenomenal field. Most arguments which have

been brought forward against their truly perceptual existence seem to be influenced by the ineradicable tendency which we have to take percepts as pictures of physical realities, if not as somehow identical with them. But no physical sequence of tones has the "minor"-quality. Still, "minor" is an objective property of certain objective auditory events. That the basis of all argument about such questions has been essentially changed by von Ehrenfels and by Gestalt psychology is sufficiently known at present. Therefore I may refer to the literature for more detail. This does not mean, however, that, admitting such (independent) tertiary value-qualities, we should sacrifice the subjectivistic theory altogether. It may still be right within certain limits.

The third possibility seems altogether strange at first. It will nevertheless occupy us later.—As to the second interpretation it is the path which the subjectivistic theory should follow if, confronted with ample evidence of objective value-attributes, it wishes to preserve its own character. These, the theory would have to say, are products of our acts of interest. And doubtless there are such cases. Even to be a goal in general seems to give a thing a new flavor. Not only is it the end, the terminating part of a circumscribed context, comparable to the edge-quality which a line assumes when a closed figure stands out from the ground. It also begins to dominate in the objective region of the field, to become its center, however unimportant, visually for instance, it would be otherwise. There are cases in which this goal-quality may survive the most radical changes of the object. In dreams it frequently happens that we find ourselves in pursuit of a goal which gradually becomes so remote and unclear that finally not even a shadow of an object-image remains.

In this case the object is nothing more than a mere something; and still it may have goal-quality.

To be more specific and perhaps more convincing: After many hours on skis in a sharp frost we come home, and before us there is brown, hot, fat meat just brought in from the kitchen. Can anything look more appetizing than this meat? This is when we are hungry. A short time afterwards—we have eaten too much and too hastily—it may be difficult for us even to stay near by when precisely the same kind of meat is put upon the table for late-comers. It does not look neutral now, it looks decidedly repulsive. And have we not enough witnesses among the literary libertines of all ages who describe the terrible change which after a conquest transforms charm into something quite neutral, if not slightly unpleasant. In both examples, when the interest changes with satiety, the aspect of the object changes as though from one end of a scale to its zero-point and beyond.

So far we are in agreement with Professor Perry. "That feeling," he says, "does somehow color its object is an undeniable fact of experience, and a fact recognized by common speech in so far as all of the familiar feelings assume the form of adjectives." [19] But he is not inclined to accept this objective aspect of interest as genuine: We cannot possibly localize the red of an object in our self; this is therefore a truly objective quality. The "tertiary qualities" on the other hand yield, he believes, to an effort of attention. When we try hard enough we find them separating from the object and tending to unite with the self.[20]

I am afraid that with this argument we approach the procedure of Introspectionism. To the Introspectionist

[19] *Ibid*, p 31.
[20] *Ibid*, p 32.

certain phenomena appear as surprising and therefore suspect. In such cases he asks attention to help him find the real sensations. Perhaps attention is successful, in so far as the disturbing fact disappears. Supposing that the change be in the direction of a more customary phenomenon, the Introspectionist will now say that he has found the real fact. More and more psychologists are becoming convinced that they are not entitled to apply this procedure. If, in an analytical attitude, I find an overtone in a clang which before was phenomenally a completely unitary sound, then my analysis has not corrected an error, an illusion; it has changed one genuine phenomenon into another. Again, if I direct my attention upon some happy feeling in order to find out what it is really like, the chances are that I shall destroy the feeling. All "tertiary qualities," too, may be treated in this way and some of them thus changed or destroyed. But it does not follow that their previous existence was in any sense illusory. That some qualities, e. g., colors, will often show more resistance than many "tertiary qualities" does not decide the point. A bar of steel is not destroyed when we beat it with a hammer, china is. Still china is as real as steel. We might in fact almost deduce from the theory the consequence that such "tertiary qualities" should change or disappear, if we look upon them long enough with the cold scrutiny of scientific analysis. Supposedly they are the objective-looking correlates of definite interest-attitudes. Instead of these we introduce the attitude of sober analysis. From the standpoint of the theory it would be surprising if they should remain unaltered under these circumstances.[21]

[21] In its general form the argument against the "attention-test" applies also, if, as I believe, many value-qualities are not due to subjective interests, but are inherent in phenomenal objects independently.

But, in this last argument, I may be misrepresenting the theory. "It seems necessary," Professor Perry says, "at some point to admit that the qualities of feeling may be referred where they do not belong." [22] From the point of view of phenomenology I cannot agree. Qualities belong where we find them. And no explanation or theory can convince us that they were not where we found them,—even if it should prove possible to shift them to another place under changed conditions of subjective attitude. The question of their origin is not the question of their present location. The main point, however, is that according to this theory the "tertiary qualities" are said to be misplaced facts of subjective interest. If this were correct, there should be agreement between the "tertiary qualities" and the qualities of those interests which are directed towards the objects in question. That this should be the case in general I find it hard to admit. The charm, womanliness and loveliness which may be found in certain objects are qualitatively altogether different from the present striving of the (male) self, but also from all other interests or conations which he may have at other times. If a face looks brutally stupid, this would be a "tertiary quality" of the negative kind. Certainly the contempt and aversion with which I look upon that face do not show much similarity to this value-quality. Finally may we take a case where the "tertiary quality" is undoubtedly a product of the interest: The goal-character of any object of positive striving is not similar to the striving itself. Therefore it cannot be interpreted as misplaced striving.

We had to interrupt our analysis of subjective valuation in order to consider the objective side of the situation. We had previously come to the conclusion that in sub-

[22] *Ibid.*, p. 31.

jective requiredness one part of the field, the self, is felt as accepting or rejecting a definite other part of the field, the object. There is a question whether sometimes the object may not have value-qualities in its own right. But there is no question that in general it acquires certain new traits in so far as it is the target of those vectors. To summarize:

Subjective valuation represents a special form of organization in which a vector issuing from one part of the field is felt to accept or to reject another part. Under its influence this second part of the field acquires value-properties of an objective character.

In this formulation I have not explicitly mentioned the self as being the source of the vector. The general aspect of the theoretical situation becomes and remains more striking, if we do not mention it specifically. As soon as we specify it, the situation tends to slip back into that atmosphere of staleness and triviality in which the most essential problems of philosophy and psychology are so easily hidden.

Against the subjectivistic theory the criticism has been raised by Professor Urban that it is circular, as all other forms of relational theory of value are.[23] It describes what happens in value-situations but does not give a definition of value; it actually presupposes the existence of value. It seems to me that the task of a theory of value does not necessarily consist in the reduction of requiredness to something else. In this sense, I think, a definition of value would be impossible. The only thing we can do is to bring into full view the characteristics of a value-situation. When these have been uncovered it becomes possible to see them in their relation to other phenomena, and thus to include

[23] *Journ. of Philos., Psychol. and Scient. Meth., 13.* 1916.

the concept of requiredness in a larger theoretical structure. An attempt toward the achievement of such a larger view will be our next goal.

Whether a consistent system of ethics can be founded on a purely subjectivistic interpretation of value is not a question which we are prepared to answer here. And it need not be treated so long as there are serious doubts as to whether subjective requiredness is the only requiredness existing. But even the nature of merely subjective valuation proves that it is utterly misleading to say: facts simply are or happen This statement applies only to those indifferent facts which fill the mental visual field of many scientists since the time of Hume and the development of Positivism within the sciences. Vectors which, issuing in definite contexts, are experienced as resisting or as welcoming certain parts of a field are no less genuine facts than are those indifferent events.

III

The attempt has been made by some philosophers to objectify the relational theory of value. If the *universe* were the context within which value is determined, subjective valuation would become an unimportant matter. But how does the universe determine values? We know so little about the universe and nothing about its demands. I am afraid that, together with subjectivity, any definite basis for a theory of requiredness would be eliminated in such an attempt.

There is, however, another way of escaping a certain limitation of the theory that all requiredness is centered in the self.

In Gestalt psychology we distinguish three major traits which are conspicuous in all cases of specific organization or gestalt. Phenomenally the world is neither an indifferent

mosaic nor an indifferent continuum. It exhibits definite segregated units or contexts in all degrees of complexity, articulation and clearness.[24] Secondly such units show properties belonging to them as contexts or systems. Again the parts of such units or contexts exhibit dependent properties in the sense that, given the place of a part in the context, its dependent properties are determined by this position.

May I use an old example once more: A melody is such a context. If it is in *a*-minor, for instance, minor is a property belonging to the system, not to any note as such. In this system the note *a* has the dependent trait of being the tonic with its static quality.

Let us compare this with subjective requiredness as it appears when the cover of everyday-staleness is lifted. There is a definite context, comprising definite items in the field which are experienced as belonging to the context. There is secondly the vector which characterizes this context as a system-property of it; striving does not occur by itself. There is, thirdly, the goal-quality and often other "tertiary qualities" in the object which are due to its place in the context.

We can analyze the melody, but not in independent parts. That would be destruction of the melody. Its minor-character for instance would be lost. We can analyze the situation of subjective requiredness, but again not in independent parts, all taken by themselves. The vector—and requiredness—cannot exist alone any more than a fish can live out of water. Again, the object loses "tertiary qualities" when the context dissolves.

[24] That segregation of such units is not absolute, that it only makes them comparatively independent parts of larger contexts need hardly be emphasized.

This is agreement in all essentials. Thus, value-situations fall under the category of gestalt. This permits us to hazard one more step forward.

If those cases in which the vector issues from the self are special examples of gestalt,—is there any reason *a priori* why the self should always play this rôle? Why should other contexts not exhibit similar vectors and consequently also requiredness? No speculation can answer this question. It is a question of facts and of phenomenological observation. Therefore, instead of selecting the universe as a context in which requiredness might be determined, let us turn again to concrete and circumscribed contexts. Are there any whose general structure is congruous with the structure of subjective requiredness, but whose vectors do not issue from the self?

Once more let us remind ourselves that the self is not the physical organism, just as objects in our present connection are not physical objects. And in particular let us note that other persons are, for our present purposes, not other physical organisms but percepts, most lively phenomenal objects. To these refers our next phenomenological question: Does the self always play the dominant rôle in our phenomenal field? Undoubtedly it does not. Sometimes those other objects called other people may be much more active and important in the field than we are. Is there anyone who has never felt small and unimportant in the presence of others whom we call powerful personalities? Who has never wished to be led by another when he was at a loss what to do and saw the assured manner of the other? How many professors, actors and singers have survived their first public appearance without having felt the audience before them as something much more powerful than themselves? When, at the writing desk, we con-

sider the phenomenal world there is a tendency to choose as objective partners of our self this desk, our books and the writing paper, perhaps in imagination some other quiet things. In this case, it is true, the self is often the dominating part of the field. But is it always?

In some philosophical systems, of course, we hear about the "epistemological subject" who seems to be responsible for the existence of all objects whatsoever including other persons. Phenomenally there is no such entity since the phenomenal self is decidedly not felt to be responsible for the existence of its objects. That other subject is a construct. When we hear about its functions we soon begin to wonder how different it really is from another construct, namely, the physical organism. In any case, as a construct it must remain outside our discussion.

But other persons are not only often more important in the phenomenal field than the self. Quite as often it is not the self from which vectors reach out towards other parts of the field, for instance, other people. These persons, on the contrary, reach towards us with their demands in many cases. The police officer makes me stop at a crossing by a sign of his hand, and I obey. Somewhere on the street a poor victim of the depression extends his hand towards the self which finds it hard to resist the demand. During a party, in lively conversation, we suddenly feel that something is wrong; the others have become silent, eyes stare at the self—somebody is about to sing, and the force of society around us makes us stop and retire to a corner in embarrassment. Is there, phenomenally, a vector in such situations? Is there requiredness? It cannot well be denied. But does it issue from the self? Phenomenally it does not. Instead it arrives at the self which, as far as the vector is concerned, has for once assumed the

rôle of the target. The vector is directed toward, not away from it. And it is for the time being the policeman, the beggar, the social group from which the vectors issue. As to the rest, whatever has been said about contexts in which the self is interested in an object remains true for these other cases, if only in the contexts the self takes the place of the object, and other persons, or a group of them, take the place of the self. It will not be necessary to compare details. If there is a difference, it consists in the fact that, being a more flexible and sensitive part of the field than any mere things, the self in such a context, under requiredness from without, is apt to develop dependent properties more strikingly than an objective goal will do in the other case. Nervousness, shame, embarrassment, excitement or other such qualities besides general goalness may develop when, for instance, suddenly all other people in the room concentrate upon the self in expectation of a speech.

Being occupied with phenomenology we may postpone explanations for one more moment and add another example.

The article which X. has just published about the political situation is really fascinating. Coming home I have again started reading, and I read until gradually there is a feeling of disagreeable pressure which soon develops into my obligation to finish a certain piece of work before next month. How could I read so long! Where in this case does the vector issue phenomenally, in the self or in some object? Not in the self decidedly which, at the moment, feels hunted, driven, compelled by something else. To this extent the situation is strictly comparable to the case in which demands of other persons are directed toward the self. Only now it is an object of thought-character from

which the vector issues. People who have to write books, to prepare lectures, to open letters of probably disagreeable content, to write other letters in which they have no interest, who hate to do all these things and still say: Too bad, I must do it—do they feel a vector extending from their selves to those things and occupations, or do they feel under the pressure of such tasks? There may be a vector issuing from the self, for instance, in our examples a vector of disgust and aversion. If there is, it becomes only the more apparent that the other vector, the positive demand, comes from the objective side.

It will not be advisable to describe other instances in which the vectors in question issue from thing-percepts, but again exert their demands on the self. Though there are enough cases of this kind, they would not at this point be given adequate attention. Even the examples just described have probably strained the patience of the reader. What are they, if not instances of the well-known "pathetic fallacy"? It is the self which from its experience equips policeman, beggar, social group and expecting audience with vectors or requiring attitudes. If the subject had not made it his task at an earlier time to write the book, to give the lecture and so on, no demanding vector, no requiredness could now, even apparently, be found on the objective side of those situations or, correspondingly, of many others.

On what basis are we so very sure about this point? One reason may be given which makes us understand, to some degree at least, why demanding vectors should not be accepted as issuing from the objective side of the field, why instead their apparent occurrence should be treated as a special case of "pathetic fallacy." This reason is once more the outspoken or unintentional identification of

phenomenal objects with physical realities. The influence of natural science has accustomed us to regard physical things as totally unable to exhibit demands. Consequently, if percepts are either identical with physical objects or almost copies of them, there cannot be any demands in them either. This applies to other persons as percepts as it applies to things.—In the case of my thought-objects there is another danger. Other people cannot see them. They say that thoughts are "in me," that they are only "my thoughts." I can, besides, do much about my thought-objects whereas other people can do comparatively little about them. The consequence is again a most unfortunate vagueness in the use of the term self. It may be as obvious as possible that often I look upon a thought-object as upon something distinctly different from myself; it will still be called "a content of my self" for such reasons. If, therefore, phenomenally a thought-object should now and then exhibit a demanding vector, could there be a stronger temptation than that by which we are led to say: This is still requiredness "inside the self"? Thus it would escape our notice altogether that, with this formulation, the strictly phenomenological ground is left, that there may be phenomenally certain 'objects' which exist only opposite my 'self,' but not opposite others at the same time and similarly, and which still are not parts of the phenomenal 'self.' [25] If demands issue from them, the origin of such de-

[25] At this point I cannot agree with the terminology which has been adopted by K. Lewin in several publications and by Koffka in his *Principles of Gestalt Psychology*. Objects of thought-character are certainly functions of organic processes, but so are all percepts. If, in the second case, we have reason to distinguish phenomenal 'objectivity' from genetic subjectivity, the same reason applies to thought-objects, which may be altogether 'objective' phenomenally. Inconsistency here might easily lead to errors in theory.

mands is no less 'objective' than is that of demands which issue from other persons or any percepts.[26]

Decidedly, experience shows that sometimes vectors do issue from other persons and from objects, such as tasks, and that the self feels himself the target of many such demands. How explain the reluctance of so many to accept this observation as correct if not by these ambiguities in the meanings of such words as self and objects? Why should the observation appear as so strange or even impossible? We hear so often about the "pathetic fallacy." Why so seldom about reasons why the phenomena in question should be cases of "pathetic fallacy" and not of "pathetic percepts"?[27] Who has given the self a monopoly for demands? I could not even admit that vectors issuing from the self are always more intense; because those which arrive there, which are directed towards the self, are often quite as vividly felt as influencing, attacking, changing it.

One more word may be added for those who would not believe in any phenomenological statement, unless they see that it is compatible with "reality," i.e., physiological or physical notions. They would still tend to identify the self with the physical organism which certainly is a most active part of the world; thus, they would attribute to the self many traits which they do not ascribe to phenomenal objects since these are regarded as passive products of stimulation. But in both

[26] *Functionally* my thing-percepts are of course quite as much *my* percepts as my thought-objects are my individual property. Naive Realism believes, it is true, that a given thing-percept may be the common property of several people, and New Realism holds a similar view Such beliefs, however, seem to me untenable (*Cf* ch 4) Both thing-percepts and thought-objects are functionally subjective and may nevertheless be phenomenal 'objects' for the phenomenal 'self'

[27] I do not include, of course, those cases in poetry where human thinking and language are attributed to trees, mountains and other things. Nobody would maintain that he perceives such events there But we perceive thunder as threatening and the attitude of the beggar as demanding.

assumptions they are wrong. The 'self,' though functionally depending upon processes in the organism, is a phenomenal correlate only of a limited part of brain events. And 'objective' percepts, including other persons, are quite as much the correlates of intense processes in the same brain. That these processes, occurring in the same nervous system, should be passive copies of stimulus-patterns is certainly an idea which can no longer be seriously held. There is no reason why, in principle and in all cases, they should be much less dynamic physically than are the processes underlying the phenomenal self.

Besides, what is the thesis contained in the term "pathetic fallacy"? It is an example of those many empiristic theories which everywhere obstruct the path of the psychologist. Originally occurring in the self only, demands or other such vectors are said to be wrongly attributed to objects in the phenomenal world. By some process of association or other learning, the theory says, they have been transported from the self to its objects. Assuming that this be true,—where are such vectors now? Whence do they issue, where do they arrive? Whether the empiristic theory is right or wrong, they now issue, in such cases, from objects and are directed toward the self. If I should discover that soap which I bought in Boston was made in and imported from France, is this soap therefore in France or is it in America? There is a tendency of empiristic theorizing to give us the impression that, once the theory is applied to a fact, this fact does not remain what it was before the explanation. This at least is indeed a fallacy. If something is found to occur on the objective side of the phenomenal world, it does not lose this objectivity when we discover that, originally, the trait in question had only occurred on the subjective side. If we were to neglect phenomenal facts after an empiristic explana-

tion has been given for them, a most interesting problem would be neglected at the same time, namely: How can a vector which occurred at first only in the self be transformed by some indirect process into a vector residing in an object? Because this is what we really find. The vector is issuing *there* now phenomenally, it actually belongs to the object in question, just as before it putatively belonged to the self. It is not my vector, my interest now which I find in the attitude of the policeman, in the beggar or in the disagreeable obligation. All such subjectivity is lost. At the time I may not in the least experience a corresponding vector issuing from my side. Thus, we can say that vectors really do occur in the objective realm, and that objects are capable of being their sources. Why then speak about a "pathetic fallacy"? Unfortunately the empiristic theory does not recognize this problem.

May I use still another analogy in order to make this point clearer? Supposing that two chemical substances A and B do not form a compound directly. It may be that by first combining one of them with a third substance C, I can then produce a compound which contains all three of them, and that, from this compound, I can afterwards eliminate the auxiliary material C, so that (AB) as a chemical compound is left. It is true that, historically, without the indirect procedure there would not be the substance (AB). But is it therefore not a real substance, a real compound now? Similarly, in the phenomenal world demands often issue from objects really, whatever previous history may be responsible for it, and their general behavior under these conditions is the same as that of vectors issuing from the self.

IV

So far we have found two classes of contexts in which there is requiredness. In the first the vector points toward

the object, in the other the object is the point of origin of the vector. If, in this manner, both the origin and the target of such vectors may be objects, it will be a natural question whether these two conditions cannot occur in one and the same context, whether there are no cases in which a demand is found to issue in one object and to accept or reject another?

We see indeed quite as clearly how a man is striving towards shelter in a heavy rainstorm as we see him approaching the self in a demanding attitude. No less convincing in its objective character is the avoiding attitude of a chimpanzee who finds himself near a strange-looking thing. Even the reference of such vectors to definite objects or regions of the field as to their (positive or negative) goals may be perfectly obvious in such cases. Whether the object in question is a thing in the narrower meaning of the word or another person makes no essential difference.[28]

Awareness of vectors in similar cases has, I believe, caused Professor Tolman to include purpose among his Behavioristic categories.[29] Are we warranted, on the basis of our phenomenological evidence, in attributing striving as a biological reality to the organism of a rat? In our earlier discussion of this point we concluded that as yet there is no biological datum which would encourage such a step. Probably Professor Tolman, as a Behaviorist, would not be interested in subjective striving as an occurrence in the rat's possible but doubtful consciousness.

Others, therefore, would adopt a strictly opposite attitude and decline to accept our description, contending that it does not conform with physical and physiological facts. Though such criticism transcends the phenomenological realm, it should be mentioned in this connection. How can we possibly perceive that an animal is striving towards or away from

[28] *Cf. Gestalt Psychology*, ch. 7.

[29] E. C. Tolman, *Purposive Behavior in Animals and Men*, 1932.

an object since our retinae are stimulated by rays reflected from the physical animal's surface and from the surface of the object, but certainly not by any stimuli corresponding to a vector between them? There are no such stimuli.

On such occasions recent developments in the psychology of perception reveal their general relevance. Quite as little as for the vectors in our last examples is there "a stimulus" for any grouping in the visual field; nor is there "a stimulus" for the figure-character of certain areas as contrasted with mere ground-character, or for the minor character of a melody. Nevertheless, all these things appear on the objective side of our phenomenal field. We have been forced to realize that certain traits of percepts depend upon stimulus-constellations rather than upon definite single stimuli. One such "Ehrenfels-quality" of a perceptual situation is the vectorial attitude in which an animal is seen to strive towards an object. About the "pathetic fallacy" seemingly implied in our description enough has been said above.

Since, however, people and chimpanzees are, in our connection, only more vivid percepts than other objects, we have to ask one last question. Do we find requiredness in contexts which contain no people or animals, i.e., in contexts which are objective in the sense that they do not contain any percepts very similar to the self?

We play a simple sequence of chords on the piano. If these are properly chosen a definite key will develop. Supposing that in this key the "leading note" is introduced in an appropriate manner, a final chord following this note is not an indifferent fact in the auditory field. It may sound *wrong* or, if it corresponds to the tonic of the key, it may sound *right*. If we stop after the leading note without a further chord, the sequence will be heard as *incomplete*, with a vector towards completion. This vector usually develops during our approach to the leading note, and becomes most intense with this note. It points toward

the tonic, if no chord beyond is given; it accepts the tonic, if the tonic is given; and it rejects other notes with varying intensity according to their place with regard to the key.

In all essential respects this example exhibits the same characteristic traits which have been discussed in cases of subjective requiredness. A context forms, in it the vector develops, and definite objects are either accepted or rejected as completions. Under the influence of the vector, in the context, they acquire those dependent part-qualities which we call *right* or *wrong*. If these are "tertiary qualities," so are all the goal-qualities which we have mentioned above. And it can hardly be doubted that, in this case, these terms refer, phenomenally, to something in the tones, not in ourselves. The last chord is heard as *right* or *wrong* with reference to the auditory context. By changing the context we may easily make a note sound *right* which has sounded *wrong* before, and vice versa.

Keys, leading notes, the tonic are unfamiliar notions to many. Though the facts in question are strictly independent of any acquaintance with the theory of music, it may still be advisable to give a second example. I have chosen it intentionally from the very commonest experiences. Nobody should think that requiredness in objective contexts is a rare occurrence, a mysterious experience, and therefore doubtful.

A man has bought a suit, and now he wants a necktie. This necktie must, however, fit in with the color of the suit. In these very words there is acknowledgment of the fact that some colors of ties would appear as required by those of the suit, whereas others would not. The case is perfectly analogous to our last example with one exception, namely, that in the case of the suit and the tie not only one, but several nuances of the tie may be *all right*

or even *good*. Requiredness, then, is not always equally specific, and, incidentally, it is not in all cases equally intense.

Once more some criticisms should be mentioned. There are persons who do not seem to acknowledge such facts of requiredness, for instance, in the field of music. Does this invalidate the requiredness? Not at all. There are tone-deaf individuals, it is true, who in spite of otherwise excellent hearing cannot even understand what we call pitch. Nothing could be more natural than that, if the auditory material is different in a person, he cannot find in it the same requiredness that we find in our auditory world.—It might be said secondly that requiredness seems to change in history. So far as we can see, no minor chords were acceptable as conclusions of any music a few hundred years ago. All music had to finish with a major chord. This has changed altogether since that time. The fact cannot be denied. But whatever the historical circumstances were which produced the change, the change itself cannot alter our phenomenology. If the historical fact proves a definite subjectivity of such requiredness, it is not subjectivity in the phenomenological sense of the term. Moreover, an interpretation of requiredness which would exclude the possibility of such changes could not be acceptable. These changes are too obvious. Any system of aesthetics and ethics should contain a theory of them in connection with the problem of *valid* requiredness. But in an interpretation of requiredness as such and in general they constitute no problem. Why should objective requiredness not be able to vary, if subjective valuation does? With all sympathy for those who feel a need for valid requiredness and for a theory of it, we must not confuse two different investigations.[30]

In the same way we come upon objective requiredness in matters of knowledge or thought. The similarities be-

[30] The problem of valid requiredness has recently been discussed by Wertheimer; *Cf Some Problems in the Theory of Ethics,* in *Social Research 2,* pp. 353 ff. (1935).

tween red, blue and purple are such that the place of purple is... The context asks for completion. If, as a completion, the words are given "between the red and the blue," their meaning fits the context; they are *right*. Or again: "Things equal to the same thing are equal to each other." The last part of this statement is seen to be *right* in the context of the beginning. Any other case of correct thought might be given as an example. All would show the same main characteristics. Precisely as in the case of subjective valuation, objective requiredness means that vectors issuing in parts of certain contexts extend beyond these parts and refer to other parts with a quality of acceptance or rejection. These other parts themselves assume the dependent properties of *right* or *wrong*. Whatever other differences there may be between logic, aesthetics and ethics—and there are important differences—this general trait seems to characterize requiredness everywhere. Even timeless truth, as our last examples show, involves no exception. Probably no theory would appear satisfactory and final in which the basic contrast between mere facts and requiredness had to be interpreted differently for the case of logic on the one hand, for aesthetics and ethics on the other. We are not in a position to deal with these philosophical disciplines as such. A much more thorough investigation of particular forms of requiredness would be needed for this purpose. If our interpretation is adequate, however, it would appear altogether feasible to develop those branches of philosophy from one common principle.

But, after all, is it not subjective requiredness which in our last examples has been wrongly "referred" to objective data? *We* are disturbed when a sequence of chords ends with the wrong note. *We* do not like to look upon a necktie which does not fit the suit of its wearer. Obviously here

the self is not a neutral observer of alleged objective requiredness. Why then should these cases not be reduced to subjective requiredness?—The observation is correct to some degree. We do not remain neutral in such situations. But why should we? Among the objects which the self may have before it there are contexts of many different kinds in some of which parts appear as right or wrong, required or the contrary. This means that in such situations there is, first, an objective context with its requiredness and, secondly, another and larger context which, besides the objective context, contains the self. That one context should form part of a larger one is a fact so frequently found even within the objective field of percepts alone, that its occurrence here will not surprise anybody familiar with the psychology of perception. And just as simpler objects may affect the self as attractive or repulsive, so contexts in music or in the visual field may, *qua* contexts, either issue vectors extending toward the self or arouse vectors in the self which are directed toward the contexts. Often they will do both, as for instance when in a sequence of chords we hear a wrong note, feel disturbed, and then go to the piano in order to correct the player. If this explanation should be taken as a mere auxiliary hypothesis, too complicated to deserve our confidence, it will only be necessary to point to corresponding cases in thought. In a book we read an argument which is logically altogether wrong. Certainly it is wrong objectively. But here again we are not neutral witnesses. We feel almost offended by such an obvious mistake, and presently a big stroke of our pencil on the margin, perhaps a note as well, will make it evident enough that a new vector emerged which was directed toward the object. In this case nobody can fail to see that a subjective vector is created in the larger context while

at the same time objective wrongness is and remains objective in the argument. There are indeed few things in the world which make us so eager to interfere as wrongness in objective contexts. Too easily, in cases of aesthetics for instance, two such facts of requiredness, one objective and one subjective, are confused, one might almost say, telescoped into each other in the theorist's mind.

To summarize our discussion of requiredness: It is not the subjective aspect of requiredness in human striving and interests which makes requiredness compatible with facts. Instead it is the observation that certain facts do not only happen or exist, but, issuing as vectors in parts of contexts, extend toward others with a quality of acceptance or rejection. That in many examples such vectors issue from the self is a relatively minor point. Its discussion does not belong to the interpretation of requiredness as such; it belongs, rather, to the geography of requiredness, in which the problem is: where do we find the contexts in question? By the same token subjective requiredness loses its apparent commonplace character. Its essential feature is still hidden from our eyes so long as the term striving, without closer inspection of its meaning, is held to solve the problem. So much is implied in facts of striving that they cannot be regarded as trivial in the present phase of psychology. After this has once been realized we shall be less inclined to regard the subjective case as particularly simple, as necessarily basic in the treatment of requiredness. There is no *a priori* reason why this should be so or why, if there are other cases, the subjective variety should be given an outstanding place. If our phenomenological attempt has been adequate, no such restriction to subjective requiredness and no theoretical accentuation of it can be defended. It seems to be a special case only. In the

following chapters, therefore, requiredness as the vector-aspect of phenomenal contexts will be taken in its general meaning. At least, it will not be regarded as a constitutive trait of requiredness that sometimes or often the vector in question issues from the self.

With these remarks we conclude our phenomenological survey of requiredness. It has been elementary throughout, and the reader may be assured that I do not regard these observations as an adequate basis for ethics or for other systematic disciplines of value. In our survey many different cases of requiredness were considered impartially, and each by itself. In actual life one requiredness is often the enemy of another, and ethics, for instance, claims that, in its field, it can settle such disputes. No basis for such a procedure has been given in this chapter. I hope very much that here again the same phenomenological method will be helpful. In fact, if one particular demand objects to another, this situation itself is one of requiredness. When studying it the phenomenologist will soon find himself in the field of ethics. But for the purpose of the present investigation we need not solve this task. Requiredness in general will be considered in the next chapters as it was in the last.

CHAPTER IV

Beyond Phenomenology

I

It is not our intention to restrict this investigation to questions of phenomenological description. To find the place of values in a world of facts is a task which has two different sides. First, there is the problem of principle: If experience gives us facts, how, in the same experience, can we find a place for requiredness? Like all questions of fundamental principle this problem can only be solved on phenomenological grounds. In a preliminary way it seems to be solved by the observation that "fact" is an ambiguous term, that not all facts are "indifferent facts," and that within certain factual contexts the requiredness or wrongness of some facts is no less real than is the existence of these facts. We have thus given to values a logical place among the facts. The second side of our task refers to a question of distribution. Experience in general has many domains. Where among these do we find requiredness as a characteristic of definite contexts? Until now our approach had to be phenomenological, so that the term "requiredness" could be given a definite meaning. Such a restriction is no longer necessary. Phenomenology is the field in which all concepts find their final justification. To what fields such concepts may be applied, once their meaning has been elucidated, is another question. And it is the aim of the next chapters to decide whether requiredness

as we have now defined it has any place outside the phenomenal realm.

As a matter of fact, even in our phenomenological analysis we could not avoid questions of distribution altogether. We were answering a question of this kind when we came to the conclusion that requiredness occurs on the objective side of the phenomenal world no less than as a correlate of subjective striving. The domain of requiredness was thereby greatly enlarged. In order to avoid misunderstandings, however, we should add one more remark at this point about the distribution of requiredness in the phenomenal realm: We can not possibly maintain that the phenomenal world is permeated throughout by requiredness. We should not be able to speak about "mere facts," this term would have no meaning for us, if in the phenomenal field nothing ever existed or occurred indifferently. Indeed, not all sequences, not all constellations of experience, contain demanding vectors. Any number of examples could be given where no such vectors are apparent. Mixed on the color wheel in adequate proportions, red and green yield a grey. Nobody who watches the disappearance of the red and the green, the appearance of the grey, could seriously contend that grey is felt to be required at this point in the sequence. Similarly, no afterimage is felt to be demanded by those experiences which it follows, however regularly such a sequence may be observed. Besides, and more generally, concrete and circumscribed contexts in mental life are not as a rule experienced as parts of one large superordinated organization, which extends all through the individual's life and in which each particular context appears as required or as wrong. In actual fact, nobody's life has this much coherence. In so far, then, as many contexts are apparently

indifferent to each other their common occurrence in one phenomenal life remains a mere fact, whatever the requiredness found within such particular contexts. From the empirical point of view, therefore, a "monism of requiredness" in the phenomenal world is not acceptable.

If the concept of "mere facts" appears as well founded in phenomenal experience, outside the phenomenal realm it even seems to have an absolute monopoly. Physical nature is generally believed to be of transphenomenal [1] existence. If we ask the physicists, they deny that requiredness belongs to the notions which they would apply to nature. Chemists need no such concept when they describe substances and their reactions. Some biologists, it is true, have begun to waver. It is hard to discuss certain phases of organic existence without using concepts which imply requiredness. And thus they speculate whether the reason for it is merely an anthropocentric attitude in our observation of organic facts or, rather, some genuine aspect of life itself. In the second case requiredness would occur as a real and as an effective side of organic processes.

We are not yet ready for the discussion of such questions. Before they are handled, preliminary problems will have to be solved. All of these refer to the relation in which we find the phenomenal world on the one hand and physical reality on the other; because on this relation our answer will depend altogether.

But is there any such relation? According to some influential philosophers our program is meaningless. To distinguish between a phenomenal world and a second world

[1] I have been told that such combinations of Latin and Greek as "transphenomenal" should be carefully avoided But how many in a million would have the slightest suspicion that here a Latin and a Greek vocable are combined in one word? May I therefore be allowed to use this most convenient expression?

of reality beyond the first is, so they say, sheer epistemological nonsense, however generally this distinction may be accepted. In order to make any statement about the properties of the second world we should have to "transcend," to leave the first. This, we are assured, is an impossible feat, because whatever observations or statements we make, they all refer to phenomenal material. Consequently there is only one world around us, and we had better refrain from calling it a "phenomenal" world, since by this very term we seem to hint at the existence of trans-phenomenal entities. If this were correct epistemology, our investigation would necessarily end here. In different forms such views have been brought forward by several exponents of modern Positivism. Since, however, similar opinions have been defended by philosophers, whom otherwise one would hardly call Positivists, I prefer in this connection to use the term "Phenomenalism." The meaning of this term is, of course, to be clearly distinguished from that of "phenomenology." Phenomenalism is a definite view of the universe—the monistic view that it consists exclusively of "phenomena"—plus certain arguments by which this view is supported. Phenomenology, on the other hand, is a *method* which we may use whether or not we share the Phenomenalist's views. As a matter of fact, it will soon become apparent that some results of phenomenological description are even opposed to the Phenomenalist's monistic belief.

Though ours is not an epistemological investigation, it seems necessary to discuss the doctrine of Phenomenalism at least fully enough to make it appear somewhat less convincing. Otherwise, what meaning could be found in the statement that requiredness occurs outside the phenomenal world?

No matter what our epistemological convictions are, we must recognize besides pure phenomenology all the natural sciences, such as physics, chemistry, geology, biology. Should we believe that the statements of these sciences refer to phenomenal facts, that to do research in physics, for instance, is only another way of dealing with percepts? This is, in fact, the opinion of not a few philosophers. As an explanation they sometimes add that the physicist is interested in particular sets of phenomena and in the relations found to prevail between these, while he disregards all other phenomena.

In general such verdicts are given too quickly to carry full conviction. What, more concretely, is the particular interest that transforms some phenomena into the subject-matter of physics? Even more essential: What feature of the phenomenal world has the power to make certain men, the scientists, altogether indifferent to by far the greater part of the same phenomenal world? What gives these men the one-sided interest which seems to be the defining characteristic of natural science? Since according to this opinion no activity can "transcend" the phenomenal world, there must be some dualism in this world itself, which leads to the segregation of the "physical" as a definite part of it. And we cannot simply assume that the dualism between the 'subjective' and the 'objective' side of the phenomenal world is fully responsible for the segregation. The physicist is no more interested in many aspects of phenomenally 'objective' events and things than he is in the phenomenology of jealousy or other emotions. He does not study "blue" as a color nor "sweet" as a taste nor "hard" as a quality of touch. Not even problems of visual size and visual shape belong, as such, to his field. How, then, are those phenomena selected with which he is really

occupied? If the physicist does not "transcend" the phenomenal world, at least he handles it most strangely; he seems to ignore by far the greater part of all its contents. The traditional solution of this puzzle is, of course, that only such percepts serve the physicist's purpose as are trustworthy signs of transphenomenal reality. If this explanation be not accepted, the segregation of physics as a particular science becomes a serious problem.

It is the weak point of Phenomenalism that it does not give us a phenomenology of the physicist's attitude and procedure. From this point of view it should be easy to show how, in concrete examples of research, the physicist always remains occupied with certain phenomena, namely percepts and also perhaps mere constructs. But in the same way it should also be made clear, first, why the physicist is only interested in a restricted number of percepts, and, secondly, what the distinguishing properties of these particular phenomena are. Up to the present time discussion of such epistemological problems has remained on a level of too much generality. As a general statement it sounds plausible that no science can deal with any but phenomenal material. On nearer inspection, however, it becomes obvious that beyond this general statement the Phenomenalistic interpretation of natural science has a definite task to solve. Far from having been solved, this task does not even seem to be recognized as a serious desideratum. Meanwhile, other people will have a feeling that the general statement is plausible only in so far as it is kept general, and that with increasing concreteness of investigation it may soon lose its apparent self-evidence.

The Phenomenalist's doctrine is monistic in character. He admits only *one* world. It cannot be denied that the defenders of Epistemological Dualism, who speak of two

worlds, one phenomenal and another transphenomenal, are also open to criticism.[2] They will more often point to the shortcomings of their opponents than discuss their own difficulties. Just as the Phenomenalist hesitates to give us, in a purely phenomenal world, a concrete interpretation of the meaning of physical research, so the Dualist tends to be slow in showing what the actual meaning of "transphenomenal" existence is, and how he or anybody else is able to "transcend" into this second world.

On the other hand, the Dualist is able to tell us an impressive story in which Epistemological Dualism is presented not as the theory of many philosophers, but as the direct outcome of research in physics and physiology. In his story, it is true, he uses—as we shall presently see—such terms as "objects," "influence of one object upon another," "sense organs," "nerves" and "brain." For this reason rash thinking might easily tend to the objection that with such terms the Dualist *presupposes* the existence of a transphenomenal world, that consequently he puts into his story at the very beginning what afterwards he represents as its outcome. This criticism, however, would not be justified. Phenomenalists have to admit that there are such sciences as physics and physiology, also that there are "objects" of many kinds, mutual "influences" between them, "sense organs," "nerves" and "brains." Their contention is not that such things are unreal; rather it is that all of them have to be interpreted as phenomenal en-

[2] I need hardly remind the reader that the views of Epistemological Dualism are compatible with the most different metaphysical theories. A Dualist in this sense holds, for instance, that percepts have to be distinguished from (hidden) physical entities which correspond to them. Whether these physical facts are ultimately of another essence than percepts (or phenomena in general) is a question which this strictly epistemological doctrine leaves entirely open.

tities. They cannot, for such reasons, refuse to listen to the Dualist's report. As a report, then, the following is common ground for both the Dualist and the Phenomenalist.

During the 17th century physicists studied a great many forms of action by which objects influence each other. The objects in question usually belonged to inanimate nature. Nevertheless the leaders of natural science were vividly interested in those other cases in which a physical influence acts upon the human organism, e.g., upon the sense organs. They could not fail to see that such action is not essentially different from action upon a second inanimate object. When struck by sound waves the eardrum will oscillate in a manner which is quite similar to the reaction of any other sensitive membrane. The human eye is doubtless a more flexible variety of a well known simple apparatus, the *camera obscura;* the principle according to which images are projected in the eye is only slightly more complicated. That after physical stimulation of a sense organ messages are carried through nerves to the brain was also a widely accepted notion as early as 1650. Since then it has become a well established fact of physiology. We know, moreover, that a human being normally has percepts only in so far as those messages arrive in higher centers of the nervous system.

At the beginning of this development the scientists tended to conclude merely that certain qualities of phenomenal objects depend on functions of the nervous system, rather than on properties of the physical objects themselves. These "sensory qualities," therefore, had to be left aside whenever statements were made about the physical objects. It was harder for them to realize that, consistently applied, the argument leads to more radical consequences. Let us repeat: An object sends out messages which stimu-

late sense organs. Thereupon other messages begin to travel through nerves toward the brain and, if this brain is functioning normally, a percept emerges. This is a long chain of processes. It is, besides, the only functional connection between the percept and the object from which the train of messages first issued. Have we any right, then, to identify the percept at one end of the chain with the object at the other end? Whatever degree of similarity there may be between one and the other, they are numerically two different facts. We cannot possibly say that what is called the physical object in this story is the same thing as the percept in question. What, then, from the Phenomenalist's point of view, is the physical object?

When the scientist tells us the story, the physical object may, for him, be represented by a percept of his. But could we say that this percept of the scientist *is* the object which causes the other person's percept? With this assumption we should soon find ourselves in difficulties. To the scientist's percept the same reasoning would apply which he applied to the other person's percept: the scientist's percept would be found to be only a remote effect of a corresponding physical object. (And we should always keep in mind that this statement would have to be acknowledged by a Phenomenalist as well as by a Dualist.) Consequently we are once more forbidden to identify the percept with a physical object. Again consequently, the scientist's percept cannot be the physical object from which the message causes the other person's percept. What, then, causes this percept?—Or, to illustrate the difficulty from another side: At a time when the other person is well aware of his percept, the scientist may turn away or leave altogether. Supposing that the other person's percept is an apple, would this apple-percept change or dis-

appear because the scientist is gone? Decidedly not. It seems to follow that the scientist's percept is irrelevant to the existence of the other person's apple-percept, and that therefore it cannot be the physical object which causes that apple-percept. What, then, causes this person's apple-percept when he is alone? Or has this percept a cause only as long as other people are present, whereas it exists without a cause when they are gone?

This situation has meant a difficulty for Phenomenalism ever since there was such a doctrine: *In observation,* quite apart from any theory, there is a dualism between percepts and their outer causes. And it is hard to interpret these outer causes as though they again were merely percepts.

The difficulty would disappear at once, if we could say that the scientist when talking about the physical apple is using his own apple-percept as the representative of something else, namely a physical apple existing independently of perception. The same distinction would then apply quite generally: There would be, for instance, bodies with sense-organs, with nerves and brains as *percepts,* and, besides, physical organisms with physical organs as *transphenomenal realities.* Space as perceived would be something different from space as a physical medium, and time-experience something different from time as an order of transphenomenal events—simply because in the case of all these notions the same procedure could be applied which we have applied here to an apple.

It seems advisable to make it still clearer why the Phenomenalist refuses to accept this explanation. He will admit that percepts are not the only material of physics. But what is added? According to him: *concepts* and *constructs,* as "field of force," "potential," "energy," "entropy," and so forth. Such notions are not arbitrarily chosen;

because in certain phases of the physicist's procedure they come in contact with percepts, the "observations" of science, and at these points percepts and constructs have to agree. But it would be a meaningless assumption that in this manner we develop more than mental structures. Of course, it is possible to speak of "physical" objects which send stimuli as messages to some entity called the physical organism, and to add the warning that all these things are meant to be "transphenomenal." Language is patient. But if the content of such statements is analyzed, it proves to be the description of certain thought-processes or thought-constructs which obviously belong to the phenomenal world. When something is said to be a "transphenomenal reality" we have before us a mental picture in which, outside one vaguely conceived region, which we call the phenomenal world, there lies a second still vaguer region, to which we give the name "transphenomenal." It is evident that, here, we only imagine two different *phenomenal* regions which do not overlap. To do this is easy enough. However, the possibility of this performance inside the domain of thought does not mean that some miraculous escape has been achieved into a transphenomenal world. Strictly speaking, Dualists are only misusing the fact that the term "trans" which has a definite meaning *inside* the phenomenal world may also be combined with the term "phenomenal" in *general*—whereupon the appearance is created as though this new combination still had a meaning. But no word has a meaning unless we can point to some case in which this meaning is realized. Certainly our visual picture of two regions which do not overlap is no such case for the meaning in question. Here we point to two *phenomenal* regions, whatever the names given to them. To point to a case in which the term "transphenom-

enal" could actually assume a meaning is impossible, because we should have to point, first, to phenomenal data—which is easy—and, secondly, to something outside this phenomenal world—which nobody could do, even if his life depended upon it. As a matter of fact, the word "transcendence" should be deleted from our philosophical vocabulary.

II

It will be obvious that in this situation any indirect arguments about the difficulties of Phenomenalism and the advantages of the opposite assumption would only seem to avoid the main issue. If Phenomenalism is to be refuted, it will have to be refuted directly. Therefore, though it may appear as an insoluble task, we shall try to give the term "transcendence" a definite meaning.

This purpose, I admit, does seem paradoxical at first, because the only procedure at our disposal is that of phenomenology: How could anything but phenomena be found on this road? The paradox will disappear, however, if we realize that in speaking of "phenomena" we are easily misled by the same ambiguity which we previously discovered in the term "facts." When thinking about the phenomenal world we are apt to consider one thing here, another there, and so forth, i.e., a number of static percepts or other phenomena, each of which seems indifferent to the existence of the others. In such a survey of the phenomenal world we simply ignore all awareness of definite contexts and relationships. As a matter of fact, it is an altogether common experience that one thing is "referred to" another or that the first "points to" the second; and such phenomena are no less worthy of our attention

than are the "things" in question. It was this remark which led us to a more adequate understanding of "requiredness." The same aspect of the phenomenal world will now help us to realize the meaning of "transcendence."

Experienced contexts, it is true, may be contained wholly *in* the phenomenal world. So far as they are, their occurrence has little to do with our problem. For this reason some arguments which have been brought forward in defence of "transcendence" seem hardly likely to impress the Phenomenalist. If we are told that, in remembering events of our youth, we "transcend" the present and are "referred to" the past, nobody would deny the fact as such. But precisely this fact as such does not seem to imply transcendence beyond the phenomenal world. Just as this world has spatial properties, and just as I may be "referred to" some far distant object, so phenomena appear in different temporal locations, and we are often directed toward some event the temporal locus of which lies far back in the past. But, unless more is said about the properties of this experience, I should not really feel convinced by the argument. It may be a remarkable trait of the phenomenal world that it contains a domain which we call the past. But, so the Phenomenalist would say, this domain remains a part of the phenomenal world quite the same. At no point, therefore, is transcendence into transphenomenal reality demonstrated by our example.—The same seems to be true of a second argument: If with all my force I press against a wall of my room, this wall does not yield in the least, and I am aware not only of my effort but also of a resistance which is stronger than I am. Although in this case I certainly experience a dynamic relationship, I cannot admit that—so far as the description goes—this relationship extends beyond the phenomenal world. Phenom-

enalists do not say that all percepts appear as 'subjective,' as dependent upon the self, or as mere phantoms. From their point of view there is no reason why percepts should not be hard and heavy. There is no reason either why, in dynamic contact with them, we should always feel superior. If it is true, therefore, that in this experience I am once more "referred to" something outside myself, it is still no less obvious that the object outside, with all its independence and resistance, remains in the phenomenal world precisely as I do.

From these unsatisfactory examples, however, we can deduce a scheme of what would be a more convincing demonstration of transcendence.

One thing may be referred to another in a great many different ways. As a simple case I shall choose a relation of size: one thing is seen to be larger than another some distance away from the first. It seems obvious here that in the reference, which extends from the first to the second, the existence of this second thing is "implied." In any concrete case a reference as such has to this extent a meaning beyond itself. One might add that, when the first thing is given, the reference contains an indication of the nature of the second, in our example, an indication of its size. It would not be difficult to show that the same applies to most forms of reference. If the second thing or "term" itself belongs to the phenomenal world, its presence and nature as part of the context in question validate the implication. But what about the reference and its implication, if the second term is not experienced as a concrete part of the phenomenal world, if, instead, it belongs to transphenomenal reality? Both the reference and the implication could remain the same so long as certain conditions were fulfilled. These are the conditions: *First,* we

should have to assume that the phenomenal world and transphenomenal reality belong to one common realm of existence, in which transition is possible from one domain to the other. The *second* condition is that "reference"—generally or at least in some of its forms—be a common functional trait of both phenomenal and transphenomenal existence, more particularly: that a reference may extend from a phenomenal term on the one hand to a transphenomenal term on the other.

So far this is mere logical analysis of a possibility. Whether its outcome has any value depends altogether on phenomenological observation. Before such observation is undertaken, however, we should know what we may expect to find, if the possibility corresponds with facts. This expectation is easily formulated: There should be cases in which a thing or, perhaps, the self is felt to be referred to some definite entity although at the same time this "second term" is not given inside the phenomenal world. The reference as such would *imply* that beyond there is such an entity; it would be felt directly as extending to a second term outside. Moreover, the fact that the reference is of a particular kind might even imply something about the nature, the status of the entity in question. In other words, there is at least one way in which phenomenological observation might give a reasonable meaning to the concept of transcendence: We might find that sometimes concrete reference is a bridge which rests on phenomenal ground on our side, and is still felt, from our side, to have a corresponding support elsewhere, although this second support is not visible from where we are. Such a second support would be a part of transphenomenal reality.

It will be sufficient for our purpose, if we discover one

single example in which transcendence is, in this way, an actual experience. We wish to demonstrate simply that, as a matter of principle, such an experience does occur, not that it is more or less frequent. One concrete example would probably show us not only transcendence as a fact, but also how it is realized despite all arguments to the contrary. Moreover, the demonstration of any case of transcendence would be a demonstration of transphenomenal reality at the same time. This is all we wish to achieve at this point.

May I return to questions of memory. One is often referred to some event of one's past life when this event is actually given as an image. In this case, as I pointed out above, the reference extends from one's present self to something temporally distant. Nevertheless, it does not seem to transcend phenomenal data. But how about this other situation? I wish to remember the new painter's name which I heard yesterday during a conversation. "Just a moment," I say to myself, "I know that I know it; I shall get it at once." Everybody has such experiences. Before a name or another fact is actually remembered there may be a difficulty, a suspense, a delay in its appearance. Nevertheless we may know at the same time that "it is there"; we feel referred, and even referred to the right thing, even though this same thing has not yet emerged into phenomenal existence. Precisely as expected, the reference extends into "darkness" beyond; and yet we feel, from our side, how, over there, it rests on adequate ground.

This description is not quite complete, however. In how far is the thing beyond felt to be the *right* thing? Whenever we try to remember in spite of temporary difficulties, some data are given phenomenally which the thing beyond

has to fit. It belongs to a phenomenal context in which its place is still left open. We are directed toward this context and, thus, we feel how the phenomenal part of the context bears definite reference to the missing part outside. It is not reference in general which in such cases implies some transphenomenal entity; it is, rather, a particular reference which extends beyond, and its implication is that the thing outside fits the concrete phenomenal context in question.

It would not be a serious objection, of course, if we were told that the wish to remember is not always accompanied by the feeling that we can do it. It should be unnecessary to repeat that we are not concerned here with questions of frequency or of regular occurrence, but rather with occurrence in principle. It would not even matter, if despite our feeling that "it is there" we should actually fail to recall the name afterwards. Lack of sufficient interest or any disturbance might easily prevent this achievement. Would our phenomenological observation of transcendence be thereby refuted? By no means. We are not maintaining that as a matter of fact certain events will follow transcendence as an experience, but that there is transcendence, and that, while it lasts, it implies transphenomenal existence.

On the other hand, just this objection is apt to call our attention to a further aspect of our example. Our experience of reference to an entity beyond is so definite that we are often clearly aware of fluctuations in the status of this entity. The delay of recall may be prolonged. In this case the situation does not usually remain stationary. "I almost got it," we say then, or "too bad, now it has slipped farther off again," and so on. In the phenomenal field there is the

incomplete context, beyond there is "it," the right name, and we experience vividly, sometimes almost painfully, how it comes nearer for one moment, recedes again, approaches once more, though still outside, and perhaps eventually emerges into phenomenal existence. Here the Phenomenalist might object that, according to this description, the approach and the receding seem to be phenomenal experiences. He would be right. They are phenomena. But they are phenomena of reference or, rather, of change of reference, in which it is directly implied that "beyond there" is some entity, the right thing, the status of which varies as the changes of reference indicate. Consequently, transcending reference has two different aspects in our example. On the one hand, it is implied in the reference that the entity outside *fits* the given, but incomplete phenomenal context, that it is the *right* thing. On the other hand, the reference varies in a manner which, for the thing outside, implies changes of functional distance, of approach to or receding from the phenomenal domain.

Many readers will remember the famous passage in which William James once described the same situation: "Suppose we try to recall a forgotten name. The state of our consciousness is peculiar. There is a gap therein; but no mere gap. It is a gap that is intensely active. A sort of wraith of the name is in it, beckoning us in a given direction, making us at moments tingle with the sense of our closeness, and then letting us sink back without the longed-for term. If wrong names are proposed to us, this singularly definite gap acts immediately so as to negate them. They do not fit into its mould." [3]

[3] *The Principles of Psychology* (1896), I, p. 251.

As a defence the Phenomenalist will perhaps maintain that even before actual recall the entity in question is not altogether outside the phenomenal world. At least a vague shadow or a dim spot might be present as a representative of the second term to which the incomplete phenomenal context is felt to be referred. It would be a fruitless endeavor if we should try to decide introspectively whether this argument has a factual basis. Fortunately, however, the argument would not be valid even if there were always such a shadow or a dim spot. The incomplete phenomenal context is felt to be referred not to a shadow or a spot as such, but to "the right thing beyond," i.e., to a much more particular and definite entity, the particular properties of which are at the time not yet realized phenomenally. In the reference to this entity it is implied that it is precisely the thing we are looking for. A mere shadow would not be felt to fit the given particular context. Thus, the presence or absence of such vague phenomena becomes irrelevant to our discussion.

In addition to this a second objection will probably be raised. After all, one might say, instead of demonstrating transcendence you are merely drawing an inference by analogy. It is a regular experience that in a case of reference there are two phenomenal terms of which one is referred to the other. In your example you find but one phenomenal term and, issuing from it, a feeling of reference. From this you conclude that even this case should follow the general rule, and that, consequently, there must be a second term somewhere.—As a matter of fact, this interpretation does not correspond with the actual characteristics of our example. It is not true that, besides an incomplete phenomenal context, I have only a feeling of mere reference plus a conviction about a rule. Whenever I am sure I "have the right thing," the *present* and *individual* reference is directly felt to lead to an *individual* second term in the particular case. So much is implied in my concrete awareness of the present reference. Moreover, the argument ignores the fact that during the delay of recall we often feel distinctly how that definite thing outside changes its readiness for actual recall from one moment to the other. I do not see any connection between such facts

and the proposed interpretation by analogy. We have no right to deprive the phenomenological nature of a situation of its more characteristic traits, until a vague residuum is left which will yield to trivial explanations.

In another connection it will be shown that, beyond the general problem of transcendence, our example has some relevance for the main question of this investigation, i.e., the problem of requiredness. As to transcendence in general, the present case is by no means the only paradigm available. In the field of memory alone we could easily add a few more which might be used in a systematic demonstration of transcendence; and I have little doubt that others could be found in perception. Since, however, it is not our aim to treat this problem generally, I shall refrain from describing other such examples. Nor are they needed. They could teach us little more than we have just been able to learn. One fact of transcendence ought to suffice to make us acquainted with transcendence as a *type* of experience. Knowledge of its occurrence seems to imply knowledge of its nature. This gives our demonstration sufficient generality for all our purposes. I do not hesitate to conclude that "transcendence" is a notion with a definite meaning, that the same is true of "transphenomenal reality," and that, in principle, we may ascribe existence to transphenomenal entities no less than to percepts and other phenomena. Consequently, it is a genuine problem whether or not requiredness occurs among the properties of transphenomenal reality.

Practically all research in natural science proceeds, I believe, on the tacit assumption that its subject-matter exists outside the phenomenal worlds of all observers. As we have seen, such a hypothesis is not meaningless in principle. On the other hand the scientist cares little for

the phenomenological foundations of this assumption. And when he tries to find the properties and laws of transphenomenal entities he does not start with such cases as give *direct* evidence of transphenomenal existence. Indeed, of these he usually knows little or nothing. His is a more naïve and a more indirect procedure. It also has a wider scope. His tendency is to jump over all epistemological difficulties and to trust his observations and his inferences to provide genuine knowledge of realities beyond all observation.

We cannot investigate how this is actually done and how the scientist's method or his results compare with direct evidence of transphenomenal existence. Knowing, however, that there is such evidence, we shall from now on assume that it is evidence of facts which lie within the general realm of physical nature—the same nature with which the scientist is occupied. As to the scientist's work we shall suppose that, in spite of the present revolution in physics, many properties and laws of transphenomenal nature are definitely known, however such knowledge may have been obtained. I am willing to trust the methods of natural science to this extent, and to surmise that those particular transphenomenal entities of whose existence we have direct transcending evidence will also sooner or later become objects of such scientific investigation.

Scientific research is directed toward inanimate objects and their behavior, but also toward organisms and the totality of those processes which we call life. (All these terms, I repeat, are now supposed to refer to transphenomenal reality.) Where in this world of nature is the locus of transphenomenal existence as we find it implied in our experience of direct transcendence? So far as our example goes—which was a case of imminent recall—we

can have little doubt about the locus in question. The distinctive trait of transcendence as we observed it, was direct coherence of function, of reference, between an incomplete phenomenal context and a transphenomenal entity. There is only one part of nature which, according to present knowledge, could in this case be so intimately in contact with phenomenal data. This part of nature is the circumscribed world of brain-events. Our conclusion will therefore be that, in trying to remember something and knowing that we know it, our reference is from the point of view of science reference to a definite neurological, or better: *neural* entity, an entity which would commonly and perhaps clumsily be called a memory-trace. I wonder whether, beyond our special example, any *direct* transcendence whatever refers, in this sense, to other than neural realities, though of course these may not always have the special form of memory-traces.

In order to avoid a misunderstanding I should perhaps add that transcendence, as we experience it in our example, does not indicate of course in what kind of transphenomenal world the 'second term beyond' is located. Nor is there in this experience of transcendence any awareness of a nervous system as a nervous system. What I wish to say is merely this: A demonstration of transcendence as such tends to justify in principle what the physicists and other scientists are doing when they ascribe transphenomenal existence to those objects with which they are occupied. Without trying here to legitimate all their particular assumptions we shall from now on treat the main outlines of their construction as accurate knowledge. Thus we shall speak of physical systems, of organisms, of nervous systems, and so forth, as transphenomenal entities. We shall, however, do more. We shall turn around, look back

upon our example of transcendence and try to give the 'the term beyond'—which phenomenally was no more than 'the right thing, still hidden, sometimes nearer, sometimes farther off'—a location among those constructs of science. If we find that this place is in the nervous system, and that, physiologically speaking, it must be a memory-trace, we are going of course far beyond the content of our phenomenological description of transcendence.

III

It would probably betray exaggerated optimism, if we should believe that after our argument in favor of transcendence the philosophical reader will feel entirely at ease. One difficulty at least has not yet been discussed explicitly. It is our next task to state and then to solve this further problem. For this purpose we shall make ourselves acquainted with one more school of philosophy.

In its attack on Epistemological Dualism, Phenomenalism has found an ally in *New Realism*. According to New Realism a dualistic epistemology leads to an unnatural and suspiciously complicated picture of the world. Any unsophisticated person, the New Realist would say, has *one* world around him. This colorful world of everyday experience exhibits all the objectivity and permanency which the exponents of Epistemological Dualism ascribe to a hidden physical world. Dualistic views, we are told, degrade the world of percepts to a merely temporal and subjective affair, and thus contradict common sense which finds itself in immediate contact with objective reality. New Realism takes sides with common sense. The world as we have it directly before us is, according to this doctrine, the physical world no less than it is the world per-

ceived. It should be possible, therefore, to find another explanation for those facts because of which the Dualists separate percepts from real objects. If we could be Realists in a more naive way there would be no need for constructing a foreign world beyond.

It is obvious that New Realism, though by no means identical with Phenomenalism proper, has at least some points in common with this view. "Can we transcend the phenomenal world?"—"A meaningless question," answers the Phenomenalist. "Why," asks the New Realist, "should I attempt anything so unpleasant?"

In a powerful book,[4] Professor Lovejoy has thoroughly examined the different forms in which, on the basis of this criticism, positive monistic theories have been developed. They all try to show how, in our thinking about physical objects and about percepts, partial or total coincidence of both can be obtained without a contradiction. Professor Lovejoy, however, is led to the conclusion that, if we develop such theories consistently, all of them give a picture of the world which is incomparably more complicated and even more startling than the picture given by Epistemological Dualism. An unsophisticated witness would be much more astonished by the results of Epistemological Monism than he could possibly be by the other view. I find great force in Lovejoy's arguments, and I accept his statement: "That this goal—of New Realism—has not been and cannot be attained is one of the clearest results of the philosophical discussion of the past twenty-five years."[5]

To a certain extent, a demonstration of transcendence means an argument against New Realism no less than

[4] *The Revolt against Dualism* (1930).
[5] *Ibid.*, p. 61.

against Phenomenalism. To the same extent our example of transcending reference may also have some value in a critical discussion of New Realism. Yet the case in question belongs to the field of remembering; and the main issue between New Realism and Epistemological Dualism is the interpretation of *percepts.* In this field it is the apparent independence, the objectivity and the external location of thing-percepts which makes it difficult for many to admit that such things are no more than percepts, not the real and independent physical things at the same time. A fully satisfactory answer to the New Realist's claims will only be given if such characteristics of percepts are clearly explained by Epistemological Dualism. I am not convinced that this has been done even by Lovejoy.

Among these characteristics, external location of percepts is by far the most impressive. Epistemological Dualism insists on the genetic or functional subjectivity of percepts, on the fact that all percepts depend on processes inside the organism. As the physiologist would put it: "An object sends out messages which stimulate sense organs. Thereupon other messages begin to travel through nerves towards the brain and, if this brain is functioning normally, a percept emerges." [6] It seems to contradict this view that, allegedly founded on processes in my interior, such percepts as tree, house, cloud, moon and thousands of others are clearly localized outside of me.

When we survey the history of philosophy and scientific thought during the past century we find that some of the ablest minds are given to serious speculation about this paradox. Both E. H. Weber and H. von Helmholtz whose work contributed so much to the foundation of psychology as a science tried to find an explanation. Philosophers

[6] See above, this chapter, p. 109 f.

from Schopenhauer to Whitehead seem to have been convinced that the genuine, the original, location of percepts must indeed be somewhere in our interior. That we do find them not there, but outside, may easily have been the strongest psychological motive in the origin of New Realism. Only a very few authors, mostly men of great phenomenological power, have been able to recognize the apparent puzzle as what it really is: a most unfortunate pseudoproblem produced by inconsistent thinking. Such men were E. Hering, the physiologist, and E. Mach, the physicist and philosopher.[7] It will help us to explain Epistemological Dualism, if we show that external location of percepts does not even contain a problem, much less a paradox. I have tried to do the same before,[8] but not simply enough and therefore not successfully. Another attempt seems required for two reasons. Quite apart from the present investigation it is almost an embarrassing fact that even great philosophers should still be influenced by a mere pseudoproblem. Besides, I wish to free this present investigation from any shadow which so deplorable a misunderstanding might throw on Epistemological Dualism.

At least since Schopenhauer one explanation has generally been given which is entirely unacceptable because it adopts the faulty premises of the pseudoproblem. According to this theory percepts would really be localized in our interior, if the mind, conscious or unconscious, were not so much given to causal thinking. All these sensory experiences, the mind seems to say, must come from somewhere. It will be a most convenient procedure, if I

[7] *Cf.* also W. James, *The Principles of Psychology* (1896), II, pp 31 ff.

[8] *Cf* *Gestalt Psychology* (1929), pp. 224-233 —A more thorough treatment of the same question may be found in my article, *Ein altes Scheinproblem*, in *Die Naturwissenschaften*, *17* (1929).

can trace them back to their several origins in space, and if I can then assign to them such outer locations.—Other authors would give a similar explanation in less intellectualistic terms. So far, however, as they feel that location of percepts inside ourselves should be expected, and that therefore external location must be explained, their theories are practically equivalent to Schopenhauer's amazing assumption of mental projection.

We shall now give the interpretation which follows from the point of view of Epistemological Dualism. In doing it we shall make use, of course, of all the notions which are characteristic of this doctrine. Consequently we shall speak of phenomena on the one hand, of transphenomenal realities on the other. And our main task will be to avoid ambiguities which arise from the fact that even in philosophical language many words are used in two meanings, one phenomenal and the other transphenomenal. If we give sufficient attention to this circumstance, the apparent paradox will evaporate.

1. If we say that *percepts depend on processes inside the organism*, what does the word "organism" mean in this connection? Evidently the physical object the properties of which are investigated by anatomists, by physiologists and by neurologists.—Is this physical object a percept? No, according to Epistemological Dualism it is, like all physical objects, a transphenomenal entity; correspondingly the processes in question are transphenomenal events. It is not an argument against these statements that, in the investigation of the organism and its functions, the anatomist, the physiologist and neurologist rely on percepts. Precisely as the physicist investigates transphenomenal entities and still uses certain percepts as his primary material, so his colleagues in biology study transphenomenal facts

although the primary material of their observations consists of percepts. Unfortunately we do not usually give different names to the organism as a transphenomenal entity and to the body as a percept, just as a piece of silver is called silver, whether we think of the physical object or of the corresponding percept.

From this paragraph it follows that when thing-percepts and other phenomena are said to be genetically subjective, to depend upon processes inside the organism, the term "inside" refers to a physical, a transphenomenal fact.

2. If I say that *thing-percepts are located outside myself,* what does the word 'myself' mean? Obviously something of which I am aware in the bright daylight of direct phenomenal experience. This commonplace 'self' is the thing to which we refer in such statements as "I stumbled," "I was sitting on a chair," "I could not walk any faster." When we use such expressions in everyday life we do not think of transphenomenal entities; nor do we refer to our own personality, our self in a more refined sense of the term. Rather, we mean a definite *percept.* In order to distinguish between this percept and the organism as a transphenomenal entity we shall from now on call the percept 'body' and reserve the term organism for the corresponding transphenomenal entity. That in this sense the 'body' is a percept can hardly be denied. Just as I see, hear and feel other things which I call my percepts,—so the thing which from now on we shall call my 'body' is to some extent given in my visual field, may be heard and may also be felt. Among other percepts it occurs as one more percept—to be sure, one of particular interest. It does not matter in our present discussion that the sensory data which constitute 'my body' are of a more varied nature than those which constitute most other percepts.

From this paragraph it follows that, when thing-percepts are said to be located 'outside myself,' the term 'outside' must refer to a strictly phenomenal fact: In phenomenal space thing-percepts are as a rule localized 'outside' one more percept which I call 'my body.'

It follows from both paragraphs taken together that, if thing-percepts depend upon processes "inside" and still are localized 'outside,' the term "inside" and the term 'outside' refer here to different entities, the organism and the 'body.' They also refer to different spaces, the first to physical space and the second to phenomenal space. Consequently the two statements do not contradict each other.

In case this is not yet obvious I shall repeat the argument in other words. According to Epistemological Dualism *all* individual percepts, those which we call things as well as the percept 'my body,' depend on transphenomenal events in a transphenomenal entity, the organism. Even the fact of such dependence, however, is in a simple perceptual situation not a matter of phenomenal awareness; only some indirect procedures and conclusions of physicists, physiologists and philosophers lead to the conviction that there is such a dependence. On the other hand, all those percepts appear in a general phenomenal medium, phenomenal space, and as a rule they are there localized outside each other: 'the tree,' for instance, near the 'house,' the 'cloud' and the 'moon' above—and 'myself' rather far from these things, although 'my feet' are in touch with the percept 'ground.' It is certainly no more surprising that the other percepts appear outside the percept 'my body,' than that the 'moon,' the 'cloud' and the 'tree' appear outside the percept 'house.' Conversely, instead of expecting to find the 'tree,' the 'house,' the 'cloud' and the 'moon' inside 'myself,' I might quite as well expect to see 'myself'

inside one of the thing-percepts 'tree,' 'cloud' or 'moon.' Without exception we are dealing here with percepts which have a definite phenomenal location relative to each other, and there is no reason whatsoever why in phenomenal space the percept 'myself' ('my body') should not in principle play the same rôle as that played by other percepts.

Thus, astonishing though it may appear, the problem by which so many have been disturbed is indeed no real problem at all. This becomes obvious as soon as we distinguish between the organism as a transphenomenal entity and the 'body' as a percept. To the first refers the statement that all percepts depend on processes inside the organism—where even the word *depend,* just as *inside* and *organism,* has a transphenomenal meaning. To the second, the 'body'-percept, refers the sentence that things have places 'outside myself'—where all words point to phenomenal facts and 'outside' is a phenomenal relation in phenomenal space. Only if we fail to see that one statement is about relations in transphenomenal space (including the organism), while the other is about phenomenal relations in phenomenal space (including the 'bodily self')—only so long can we believe that these statements contradict each other. Confuse the organism with the 'self'-percept, fail to distinguish between physical space and phenomenal space, and you have the great paradox: "Inside" predicated there contradicts 'outside' found here. I have to add only that the paradox disappears without the help of any special hypothesis. It simply vanishes before consistent thinking on the accepted premises of Epistemological Dualism.

The argument as given does not strictly speaking need supplementation. For the sake of completeness, however,

and in preparation for further discussions we shall take one more step which will bring us in touch with science, namely, with neurology. From the last paragraphs one is naturally led to two questions: Why should percepts be so nicely distributed in phenomenal space? And, how more concretely do phenomena depend on processes inside the organism? It will be obvious that these questions belong together. They express the need for more insight into the relation which prevails between percepts in phenomenal space on the one hand and neural events inside the organism on the other.

In trying to give a preliminary answer, we had better simplify the situation. The field of percepts usually contains data of many sense-modalities: visual, auditory, tactual, olfactory, kinaesthetic and so on. There is but one phenomenal space in which they all appear; and order in this space is, as a rule, one common order for all of them, so that, for instance, I hear a car starting where I also see it. Why that should be so is still a matter of controversy with which I do not wish to burden this discussion. For this reason the following account will be restricted to data of one sensory modality only, namely, that of visual percepts. Our problem therefore is: How is the spatial distribution of visual percepts connected with their dependence on transphenomenal events in the organism?

Without trespassing on hypothetical ground we can give the following preliminary answer. On the retina of the eye, images of physical objects are projected in an orderly manner. The distribution of images is similar to that of photographic images on the sensitive film in a camera. In consequence of such stimulation, nerve-impulses from various points on the retina begin to travel along the fibers of the optic nerve and then along more

central neurons. Eventually they arrive in a definite part of the cerebral cortex, the striate area, and there cause further processes which seem to be the final phase of purely visual function. According to present knowledge visual experience of human beings depends on these processes. The most important point, however, is that the impulses on their way to the cortex and the final processes in the cortex are no less well distributed than is stimulation on the retina. At the present time evidence is overwhelming for this fact both in human pathology and in experimentation on infrahuman mammals. Whatever the reason may be, impulses arrive and processes develop in the striate area of the cortex as an orderly pattern. Moreover, the spatial properties of this pattern are to some degree akin to those which the pattern of images exhibits on the retina.

This statement will acquire a more definite meaning if we add a few anatomical data. The brain, as everybody knows, has two hemispheres which are to some degree separated by the median fissure, but are at the same time connected by several structures, first of all by the fibres of the *corpus callosum*. The cortex of each hemisphere contains a striate area. In the case of the human brain these areas are located on the dorsal side of the brain where the cortex of both hemispheres bends and disappears into the median fissure. Here the two striate areas face each other across this fissure. Only small portions of the areas remain on the outer surface of the hemispheres, covering their posterior poles.

Anatomically the extension of the visual cortex is well defined by a histological peculiarity. Wherever this part of the cortex is cut vertically to its own surface a white stripe appears

in the otherwise grey tissue. It is this white stripe which has given the visual cortex the name of "striate area."

Let us suppose now that only one eye of a person is functioning. On the retina of this eye images of outer physical objects will be projected, and presently nerve impulses will begin to travel along the optic fibers. As I said before, at their arrival in the cortex their spatial distribution will correspond to a certain extent with the pattern of retinal stimulation. More specifically: those impulses which started from the left half of the retina arrive in the striate area of the left hemisphere, while those which come from the right half of the retina take their course to the right striate area. Under normal circumstances both eyes function. The images of objects are then projected upon the retinae of both eyes. For the second eye, however, the same rule holds: impulses from its left side arrive in the left striate area, impulses from the right half in the right striate area. For simple reasons of retinal projection such processes of the two eyes as correspond to the same objects in physical space outside are conducted in this manner to approximately the same parts of the visual cortex. A physical object to one's left will be projected on the right side of both retinae, and so forth. Since the impulses which correspond to the two images of the object arrive at approximately the same region of the visual cortex they may perhaps arouse one single cortical process there. Certain facts seem to indicate that often this is really the case, although here we reach the limits of actual knowledge. On the other hand, it is definitely known that between the distribution of processes in the visual cortex and the distribution of images on the retinae there is correspondence not only as to halves, but also as

to smaller parts. In this sense neurologists are doubtless right when they say that the visual cortex, i.e., the two striate areas taken together, represents a "cortical retina."

May I offer a concrete example. (As has been done consistently in the last three paragraphs all terms will be used in a physical or transphenomenal sense). A man sits at his desk on which there are a pencil and a ruler, several inches apart. With a given direction of his eyes an image of the pencil will be projected on certain parts of both retinae and an image of the ruler on other parts. Between these images the surface of the table will be projected as it will also be around these images. From what we know about the "cortical retina" and its connection with the eyes it follows immediately that in the striate areas there will be two separate processes in different places, one corresponding to the pencil and the other to the ruler.[9] Between these processes and around them there will be other processes which correspond to the surface of the table. In this statement there is, I insist, no speculation whatsoever. It simply expresses, for a particular example, what is definitely known as a general rule. And it follows that, *to a certain extent,* the spatial relations not only of retinal images but also of the corresponding physical objects themselves are, in the visual cortex, represented by analogous spatial relations.

The physical situation, however, will usually contain more components than those which have as yet been men-

[9] In order to simplify matters I am here omitting the fact that of each object there are in our case *two* images, one in each eye, which might, when corresponding impulses arrive in the cortex, produce two processes for each object Those who feel disturbed by this situation may either assume that actually these two similar processes in approximately the same place will become fused; or else, they may consider the case where only one eye is open. This case is quite sufficient for a demonstration of what we want to prove

tioned. Let us assume that in our example the man has put his hands on the edge of the desk. So far as reflection of daylight from their surfaces is concerned these hands are physical objects just as are the pencil and the ruler. Consequently, if the hands are in a natural position before the rest of the organism and before the eyes, images of his own hands will be projected on the retinae of the man. Supposing that on the desk the positions of the hands are different from those of the pencil and the ruler, these retinal images of the hands will have different locations from those of the two things. Since all these images do not differ from each other in principle, nerve impulses will start where the images of the hands are projected precisely as other impulses issue from the retinal images of pencil and ruler. Again, in the visual cortex of the man there will presently be processes which represent his hands, and these processes will have locations of their own, different from those where the pencil and the ruler are represented by their processes. Now, as a rule, the hands will not be the only parts of the organism which in this manner stimulate the eyes of the same organism so that eventually corresponding processes occur in the visual cortex. Rather the retinal images of the hands will be parts of more extended images in which projection of the forearms and perhaps, more peripherally, even the trunk and some portion of the legs may be included. Since retinal projection preserves to some degree the spatial order in which, outside the eye, the objects are distributed; since, furthermore, a similar relation holds between the retinal projection and the distribution of cortical processes, the neurological situation is simple enough: In their totality the processes of the visual cortex may be considered as a picture of the fact that here an organism is confronted

with a number of objects, that these objects have certain spatial relations relative to each other and relative to the organism. At least in so far as the objects and the organism are *outside* each other there is agreement between their distribution in physical space at large and the distribution of corresponding events in the narrower physical world of the brain. For, in some part of the visual cortex the visible surface of the organism is represented, and in other parts of the same area we have those processes which represent the objects outside the organism.

Between the cortical representation of the visible organism and that of other objects there is of course *no gap*. If the hands, for instance, lie on the edge of the desk, the retinal image of the desk will extend between the images of the hands and those of the pencil and the ruler. The same will apply to the processes which, in the visual cortex, represent the hands or other parts of the organism, the surface of the desk and those objects. Still, all these parts of the situation remain outside each other, as processes in the "cortical retina" no less than as objects.

There is no essential change in this respect, if the eyes turn to a new point of fixation, or if the organism moves as a whole. In both cases retinal images both of other objects and of the organism will generally change their locations, some images will disappear and others will take their places. The same will be true of corresponding cortical processes. But since all the physical objects remain outside the physical organism, their retinal images will always remain outside the image of the organism; and corresponding cortical processes will remain outside the process by which the surface of the organism itself is represented.

Not all philosophers are well acquainted with this part of neurology. I repeat, therefore, that my description does not in any essential point contain mere assumptions, al-

though it ignores several problems with which at present we are not yet concerned.

Among the transphenomenal entities which have been mentioned in these paragraphs the processes in the visual cortex are, for our purposes, by far the most important. So far as we know, on these processes visual percepts depend most directly. If a circumscribed but not too small part of the striate areas is destroyed, no visual perception is possible of such things as would otherwise be represented by processes in that region. Any essential disturbance of the "cortical retina," by pathological factors or by wounds, leads to alteration of the phenomenal visual field. It seems safe to conclude that normal vision is based on normal function of the same cortical region, more concretely, that particular visual percepts as they appear in the visual field depend directly on particular corresponding processes in the visual cortex. Such terms as "depend on" and "based on" should, of course, be used with caution. They are meant to express the fact that certain things, the visual percepts, exist, disappear or change in correlation with the occurrence, the disappearance or the alteration of corresponding brain-events. A relation of this kind may still be interpreted in several ways. But in our present connection there is no need to choose between such different possibilities.

We come back to questions of localization. Part of the organism itself, we have found, and physical objects before it are represented in the visual cortex by processes the distribution of which corresponds roughly with the fact that those other objects have locations outside the organism. If, now, on all such processes visual percepts are based, is there any neurological reason why phenomenally the thing-percepts should be localized inside the body-percept? In

the visual cortex the processes on which the thing-percepts depend are certainly external to those on which the visual body-percept is based. Whatever else this may mean, the unfortunate expectation that 'things' should appear in the interior of the 'body' cannot possibly be derived from the neurological situation. On the contrary, so far as distribution of underlying processes is concerned, precisely the opposite, localization of visual things outside the visual body, might seem to be more plausible. And it is this spatial relation which we find realized in visual experience when we say that a 'house,' a 'tree,' a 'cloud' and the 'moon' appear at different distances '*outside* ourselves.'

Sometimes one hears the objection that after all the visual cortex, as the brain in general, is situated inside the skull; and that to this extent all the processes of the visual cortex remain interior. Thus one might expect the visual field as a whole to have an interior location. In this argument a certain fact is correctly stated, but the conclusion does not follow. Granting for a moment that mere geometrical relationships in the cortex are really essential for spatial relationships in phenomenal experience, we should at once have to add that this would apply to neural events on which visual percepts or other phenomena are based, not to anatomical structures in which such processes will never occur. So far as brain-events and visual percepts are concerned, the presence of the skull has no direct relevance. The fact that the skull surrounds the brain could show in the visual field only if in bones there were processes on which percepts would depend. In this case the visual field might conceivably appear as surrounded by such percepts. But there are no such processes in the skull. Why then should the visual field appear as locked up in something else?

Though our immediate task is hereby solved, a few remarks will be needed lest our representation of neurological facts be seriously misunderstood.

A most important point is this. The distribution of processes in the visual cortex certainly gives no support to the idea that thing-percepts should have an interior location. So much is true. On the other hand, it would be a serious mistake if we were to make the positive assumption that the distribution of different processes in different parts of the visual cortex yields, as such, a full understanding of actual relationships in phenomenal space. This warning applies generally, not only to the spatial relations between thing-percepts and the body, but also to those between two or more thing-percepts. Apart from the fact that phenomenal space is three-dimensional, while our discussion has referred to a two-dimensional pattern of brain-events, phenomenal space has numerous traits which mere geometrical distributions of cortical events will never make us understand. To give one simple example only: visual space has to some degree metrical properties. In a given case it has a sense to say on mere inspection that the distance between two things in the visual field appears equal to the distance between two other things of the field. If, now, we should suppose that in the visual cortex the same equality holds for the two distances by which the pairs of corresponding processes are separated, our assumption might be often wrong, and it would surely be wrong in certain cases. Visual distances are not metrically proportional to geometrical distances in brain-space.

On the other hand, as to topological relations of mere distance and mutual exclusion in general, there is good agreement between percepts in the visual field and corresponding cortical processes. In our present discussion this is the only relevant fact.

To summarize: Epistemological Dualism holds that percepts cannot be identified with physical objects, because

percepts emerge only after many events have happened between the objects and the organism, in peripheral parts of the organism and eventually in the brain. This view seems to lead to a paradox since, as a rule, thing-percepts appear outside our body. We have first shown that this argument is due to an ambiguity of terms. Then, in a discussion of neurological data, we have found that on this ground localization of thing-percepts 'inside ourselves' could never be expected. Much discussion between the New Realists and their opponents would probably have been avoided if more attention had been given to these sections of psychology and neurology.

In this chapter it has been explained in what sense we use such terms as *nature* and *physical world.* It will be our next task to compare nature with the phenomenal world.

CHAPTER V

The Nature of the Physical World

I

If requiredness occurred in nature, the phenomenal world and transphenomenal reality would have one essential trait in common. According to a widely accepted belief this is impossible since, so we are told, both the material and the laws of nature are incomparable with the contents and the laws of immediate experience. As a matter of principle there cannot be any traits common to the "mental" and the "physical" sides of the world.

Is this true? No, it is absolutely impossible, provided that the physical world is transphenomenal, and that we are nevertheless able to discover some of its secrets. Let us first consider nature in general and postpone the special discussion of living systems. Man has no direct access to the physical world. The phenomenal world contains all the material which is directly given to him. Thus, our approach to the physical domain will under all circumstances consist of inferences which we draw from the observation of certain percepts or, perhaps, also from other experiences; it will always be a procedure of *construction*. For this construction no building material is available but what we find in the phenomenal world. In this sense, then, it does not only become possible that in some respects nature has traits in common with phenomenal material. Rather there can not be a single trait in

nature which has not at least one model somewhere in the phenomenal world. This refers to nature so far as we feel we know something about it. How, in fact, could the process of construction give us any special or general result which was *toto genere* different from all experiences that men have ever had? Standing before the physicist's building we could and should always point to this stone or that principle of structure and should ask: Where did you get that? Besides, what is it? And the physicist would always have to point to some phenomenon as his source; sometimes, though the item might look like one thing in his building, he would even have to point to several phenomenal data in turn as being the various ingredients of one of his concepts.[1]

For a moment it might appear as though in the last chapter an argument had been given which would invalidate this conclusion. There I tried to show that "transcendence" is a definite experience, and that to that extent we are at times in direct contact with transphenomenal reality. However, such contact remains limited enough, limited to those implications which are inherent in a reference as it extends from a phenomenal term to a second term outside. In the experience of transcendence this second term as such does not become accessible, may its existence be ever so forcibly implied in the reference. As to its properties outside, they are again only given by implication. And I do not know of any case in which transcending reference would, for the entity outside, imply otherwise unknown properties. The implication is always

[1] A short time ago K. Lewin made the statement that the objects of physics are experienced "no less directly than those of psychology" (*Principles of Topological Psychology*, 1936. p. 20) I hope that Professor Lewin will find an opportunity to explain by what argument he supports this opinion.

such that an *understandable* relationship extends from the phenomenal term inside to the hidden term outside; which means that the entity outside is, in the relevant respect, comparable in principle with the phenomenon inside. In this way, therefore, we cannot escape the conclusion that the building of physics consists of such concepts as have, without exception, relatives among the phenomena. On the other hand, transcendence as I know it seems to show that, besides being the only material available, concepts shaped after phenomenal models may also be a most appropriate material. This follows from the fact that in transcending reference transphenomenal entities are implied with such properties as compare intelligibly with phenomena in each case. I do not expect to find any examples of which this could not be said. So far as we know transcendence, it seems to show directly that transphenomenal reality is akin to phenomenal experience. The physicist's problem would then be what concepts of phenomenal origin in general he should use in his construction, and what special concepts would fit best in each particular case. In his choice he is directed by observational data on the one hand and by the need for systematic consistency and clearness on the other.

Though all the material which the physicist uses in his construction is ultimately derived from phenomenal sources, his procedure may of course result in a building which, to superficial inspection, has very little in common with the phenomenal world. After the general model of phenomenal things he can construct transphenomenal objects the properties of which differ widely from those of the model. Thus, many traits of phenomenal things may be strictly excluded in the construction of transphenomenal objects. The same may happen with relations which

we find between things in the phenomenal world, but do not admit in the system of physics. It is a truism that elimination of phenomenal characteristics from this system has really marked the progress of science during the last 300 years. Urges as felt by man were once freely attributed to physical entities in action; so-called tertiary qualities played an essential rôle in early physics; common sensory qualities still belonged to Aristotelian physics when Galileo was young. They have all disappeared from the present system. From this fact alone it follows that nobody who becomes acquainted with the physicist's building will find, at first, much resemblance with his familiar phenomenal home. Its foreign appearance is, however, further enhanced by the high degree of elaboration which *accepted* phenomenal data undergo before they are finally included in the system. Probably most concepts of physics are complicated products of thinking in which materials from many places in phenomenal experience are united, so that at present it is extremely hard to say where in the phenomenal world their various sources lie. Most of all is this true of many notions which the physicist tends to identify with their mathematical expressions. Apparently nobody knows exactly how many different materials from how many different parts of the phenomenal world contribute to one such apparently simple concept as, say, "potential energy." Clearness and familiarity of the mathematical *symbol* determine, for the expert, exactly how he has to use it. It leads to accurate operations. But this does not mean that the expert can give us its legitimation in terms of ultimate phenomenal data. Some people are excellent drivers who cannot fully explain how their motor has been built. It is not even obvious or likely that all the ingredients of such concepts are taken from the

world of *percepts*. The less so since, if the concept itself is now preferably given in mathematical form, its creation may also be mainly due to mathematical reasoning. A physical world which is described in such terms cannot fail to look utterly foreign to those who habitually dwell in the phenomenal world and are little acquainted with mathematics. Even the expert may easily have the same impression since, so long as symbols and operations work nicely, he is not apt and not obliged to test their ultimate foundation. Even to him mathematics may sometimes seem to be a world by itself because its connections with the phenomenal world are not easily traced. Consequently his mathematical description of physical systems will often appear as a procedure the object of which has hardly any similarity with the phenomenal world. Still—and here we return to our main argument—the meaning of all mathematical symbols and operations rests ultimately somewhere on phenomenal ground, though, I repeat, not necessarily in the realm of percepts. Not even the modern technique of axiomatic or implicit definitions has brought an *essential* change in this respect. We may *reduce* the number of ultimate phenomenal data, relationships and what-not which give our concepts their meanings, but it is impossible really to eliminate them all.

Often nature and the phenomenal world are said to be, if not entirely incomparable, at least totally different in particular respects. Thus, some authors find physical reality mainly characterized as a realm of quantities, while all phenomena, they say, are so essentially qualitative that quantification in psychology, for instance, would necessarily falsify the phenomenal material of this science.—It seems doubtful whether we can really draw a sharp dividing line between quantitative and qualitative properties.

But even if we were able to do so, it is obvious that no quantitative traits could be found in the physical world unless this were also true to some extent of the phenomenal world. The same reason applies here as before: If there were no quantitative relations whatever in the phenomenal world, if, more particularly, none of our mental operations implied quantification, the attribution of quantitative traits to physical entities could never have occurred. The term "quantitative" would have no meaning; English and other languages would have no words with the connotation that "quantitative" actually has.

It is certainly true that what we call "counting" and "measuring" today is the outcome of a long historical development. But without any actual measuring everybody perceives that certain things are bigger than others, that it takes more time to walk from one village to another than through the first; even without counting, the poor man sees that the rich man has more cattle than he has himself. Counting and measuring, beyond such primitive comparison of phenomenal amounts, includes in all cases more complicated mental operations. These operations, however, belong to the phenomenal world no less than do perceptual situations which exhibit quantitative traits in a simpler form. Once more, if in our intellectual activities as phenomenal events there were no basis for and no tendency towards definite quantification, we could never have proceeded to as much accurate measuring as is now done every day in physics.

Even the transition from primitive comparison to actual measurement is sanctioned by direct awareness of their equivalence in principle. If one stick appears very much longer than another, this simple comparison *implies* that the second can be put side by side with the first, so that

the first extends beyond both ends or at least one end of the second. If two sticks seem to have the same length in primitive comparison, there is a *reasonable* expectation that their ends will coincide when they are put together. Such a coincidence would not be an altogether new, independent and surprising event. And this is, in fact, the principle according to which we usually *measure* the length of an object. Instead of concentrating upon phenomenal extension as such, we make observations about coincidences of points, namely, of the ends of a standard object and of points along the length which we wish to measure. In primitive comparison of two phenomenal extensions, it is true, no number could as a rule be found which would express their accurate quantitative relation. Still it seems obvious that actual measuring which yields such a number refers, though indirectly, to the same thing—extension—as we attend to in primitive comparison. Without such phenomenological equivalence in principle our ancestors would hardly have invented the more indirect and at the same time more accurate procedure. Certainly we do not leave the phenomenal world, its relationships and reasonable operations, when we develop quantitative procedures.

Though apparently equivalent in principle, primitive comparison and actual measurement will sometimes yield different results. Objects which seem to have different sizes in primitive comparison may have the same size when measured, and vice versa. In such cases the physicist speaks of "illusions" which are inherent either in phenomenal size or in primitive comparison. He prefers the method of measuring, i.e., of coincidences of points, which gives him more definite results. When points coincide in phenomenal space, corresponding points are, as a rule, assumed to coincide in physical space. That science here decides in favor of the indirect procedure and against immediate appreciation is one essential step in its

emancipation from many phenomenal data. Even so, the full meaning of measuring is not contained in the fact that, in a given case, the end of a standard object coincides so many times in succession with certain other points. For the result is given in terms of *length* of a measured distance, which means that coincidences of points and the number of such coincidences are still regarded as symptoms of something else, namely, spatial extension within which those coincidences occur. Spatial extension as thus measured is, of course, not identical with phenomenal size. As so many "illusions" show, there may be contradiction as to quantity between the two. They may also differ in other respects. Nevertheless "real length" and the indirect procedure of measuring it are still related in the same sensible manner which originally made measuring a reasonable procedure.

It is impossible to attribute quantitative traits to physical nature and at the same time to deny their existence in the phenomenal world. If, instead, we were told that qualitative differences are to be found only among phenomena while physical nature is a realm of pure quantities, this statement might conceivably be true. Science has discarded so many phases of phenomenal experience as not belonging to transphenomenal reality that complete exclusion of qualitative differences might seem to be the natural end of this process.

However, reduction of qualitative differences in science has never gone so far. When we speak about the "mass" of a physical object we mean one thing, when we speak about its "length" we mean another. Nobody has, so far as I know, ever tried to show that this is a merely quantitative difference. Still it might appear as though the physicist recognized qualitative differences between a few basic concepts only. Other properties of the physical world seem to be "reduced" to these few elementary notions: to

"mass," "length," "time" and, perhaps, "temperature." A simple example: In order to know what the velocity of a moving object is we measure the distance between two points of its course, we also measure the time which is spent on this distance, and then we divide the first amount by the second. For this reason a velocity is said to have the *dimension* length: time, more carelessly sometimes: *to be* a length divided by a time. It seems to be the belief of many that in attributing to all velocities the dimension length: time we reduce the meaning of velocity to those of length and of time. Since in a similar manner all concepts of physics are given dimensions in terms of those three or four basic concepts, it would follow that only these are *essentially* different, while from them the meaning of all the others could be derived. To this extent the number of irreducible physical *qualia* would be extremely small.

I do not think that this is a correct conclusion. Take the example of a velocity. It is true, we compute a velocity by dividing the distance traversed by an object by the number which indicates how many seconds it has spent on the way. But, on the other hand, if we divide a distance by a number which indicates a certain duration, the result need not be a velocity. I may, for instance, look at a line for 5 seconds and find by measurement that the length of the line is 10 inches. Dividing 10 by 5, I get 2. This 2 is, of course, not a velocity. Though trivial enough, this example shows how hasty we should be, if concentrating on measured quantities and a simple mathematical operation, we were to forget the background which gives a meaning to both the measuring and the operation. Many quotients may be computed which would have the dimension length: time, but only in particular cases would the quotients

have the meaning of velocities. It can therefore not be true that the concept "velocity" is *defined* by its dimension. In fact, when we compute the velocity of an object it is not some distance in general which we measure, but the distance through which the object moves with the velocity in question; and it is not some duration in general which we observe, but the time which it spends moving with this velocity through the measured distance. Thus, the concepts "movement" and "velocity" are used, presupposed and needed to point to those special lengths and times which make the simple quotient a measure of velocity. Consequently the quotient as such is not a definition of velocity; if velocity has the dimension length: time, velocity is not thereby reduced to these simpler *qualia;* "movement" and "velocity" remain unreduced basic concepts even in physics.

On the other hand the usual measurement of velocities in terms of distance and time has a good phenomenological reason. In the concept of velocity as such it is implied that the greater the velocity of an object, the shorter the time which it spends on a given distance. It is this implication which gives our method of measuring velocities its sensible phenomenological basis. In this respect the connection between velocity as such and the measurement of it is strictly comparable with the relation between extension as such and its measurement by the method of point-coincidences. And again, as in the case of extension, the measurement of velocity will often contradict our more direct and primitive appreciation of speed. In this situation the physicist trusts his measurement and discards the direct perception as illusory. This means that, once more, a "real" trait of nature is being distinguished from a corresponding trait of the phenomenal world. Nevertheless in the real world the reasonable connection between velocity as such and its measurement is preserved. It gives the measuring its meaning.

There is a further argument against the attempt to interpret the dimensions of physical concepts as definitions of these concepts. In mechanics potential energy has the same dimension as kinetic energy. At least in this case, therefore, the dimension fails to give a difference between two meanings which doubtless have to be distinguished in physics. It would be easy to add other cases where the same is true. Conversely, one single concept of physics may have different dimensions according to the context in which it is measured. Thus, electric charge has one dimension when measured electrostatically and another dimension when measured electromagnetically. Nobody would contend that electric charge is one thing in the first case and another in the second. Again we might enumerate other examples to which the same argument applies. Or also: Electrostatically measured, electric capacity has the dimension of a *length*. Could anyone seriously believe that the meaning of the term "electric capacity" can be fully reduced to the meaning of "length" as such? It follows once more that the dimensions of physical entities, though they have great value in other respects, are by no means definitions of these entities. Again, therefore, if the dimensions of all physical notions contain only a few simple concepts, the meanings of those notions are not thereby reduced to these simple concepts.

II

The erroneous interpretation of physical dimensions as definitions would hardly be defended by any authority in the field. At least the last argument against it is too obvious. The misunderstanding occurs more frequently among stu-

dents and among those who take an interest in physics from the outside. Still, it is significant; because in a primitive form it represents a general trend of thought by which even the greatest scientists of our time are strongly influenced. We may call it the Positivistic trend in physics. Positivism has various other forms with which the present discussion is not concerned. The opinion, however, which we shall next analyze refers directly to our problem.

For a long time physicists have been so busy with the discovery of relevant facts and laws that they have not been able to give much attention to their own procedure, its premises and its final scope. In this respect a radical change has occurred. One scientist after another occasionally takes a few steps back from the building so that, from a distance, he may better see what it looks like as a whole. Fully aware of the fact that in the beginning of the construction much material was being used which later on had to be discarded, such critics of their own work will naturally search for more valueless ingredients that should be eliminated. The more complete the building, the stronger also the wish to remove all remnants of mere scaffolding which still disturb the clear outline of the edifice. It is in fact an intense urge for neatness, pure design and lucidity which marks the present era of physics. This tendency became particularly strong when Einstein showed that even our common concepts of space and time cannot be applied to the physical world without a radical correction. Since then several other concepts which before had been regarded as fundamental in science have lost much of their prestige. As things are now one can sometimes hardly help feeling that such efforts might be *too* successful, that with the scaffolding the building itself might dis-

appear and leave an emptiness behind, which would certainly satisfy even the highest demands of clearness.

Among the most remarkable achievements of self-criticism in physics is the thesis that all observation in physics is reduced to coincidences or "pointer-readings." A pointer-reading is the name which the physicist gives to the phenomenal fact that the pointer of an instrument is seen to coincide with a line on the instrument's scale. "By the dropping of redundant sense data," says Eddington, "we have reduced our observational material to pointer-readings, or more generally to coincidences." Nor is this all; for, since such observational material is so very much the essence of science, all sober statements about the physical world itself will be given in terms of pointer-readings; in other words, "our knowledge of the objects treated in physics consists solely of readings of pointers and other indicators." In the case of a particular object or situation "there is always the triple correspondence—

a) a mental image, which is in our minds and not in the external world;
b) some kind of counterpart in the external world, which is of inscrutable nature;
c) a set of pointer-readings, which exact science can study and connect with other pointer-readings."

It follows that "none of the images which constitute our sensory perception are applicable to the physical world" —, with the sole exception, of course, of coincidences themselves, which are primarily phenomenal data or "images." So far as perception is concerned, therefore, the physicists seem to be giving us a picture of their world in which scarcely any resemblance is left with phenomenal material. I agree with Eddington when he says that his view

is essentially the current scientific doctrine.[2] He realizes the radicalism of this doctrine so vividly that he makes several attempts to console the layman who might find the world of mere coincidences rather dull and physics as a science of coincidences fairly disappointing. The main redeeming feature of a physical world in which only coincidences are known is the fact that such coincidences can be arranged in a "scheme of relationship, or a *structure*."[3] The physicist somehow connects pointer-readings, and the results of this process are what we call "physical laws."

If we postpone for the moment the questions which naturally arise from the last statement, we have to admit that the physicist's building seems now to be extraordinarily neat—not only because it contains so very little and such thoroughly homogeneous material, but also because this simple material as such, the coincidences, will by its severe precision please even extreme Puritans.

However, something must be wrong with the thesis that pointer-readings and other coincidences are the only perceptual material which the physicist uses in his research and in the construction of his building. Here is the story of some real facts in mere pointer-readings:

4; 1; 1887; 60; 5; 52, 7; 19.

The reader will be unable to understand the story. As a matter of fact, it is a simple report about the author's childhood:

"He was born, as the 4th child of his parents, in the 1st month of 1887, under the 60th degree of northern latitude. When 5 years old he went to a town under the 52nd

[2] The above quotations are from A. Eddington's *New Pathways in Science*. 1934. pp. 16, 18; and from *The Nature of the Physical World*. 1928. pp 251, 258

[3] *New Pathways in Science*, p. 16.

degree of northern latitude where from his 7th till his 19th year he went to school."

What I have added now does not refer to colors, feelings and other phenomena which really seem to have no place in modern reports on physical facts. It refers to those notions which give each pointer-reading a meaning, and without which they are nothing but indifferent numbers. It refers besides to the manner in which the pointer-readings "cohere" or are to be connected so that we get a story and not a mere aggregate.

Let us apply this to physics proper. It is a minor point that, in general, coincidences have physical significance only, if they are observed with reference to a scale, in terms of which the point in question is found to be "7" or "18.4" and so on. If on an instrument such numbers should be found right underneath or above the point in question, such percepts are, of course, only substitutes for the fact that others, those who made the instrument, have in their world of percepts looked upon this point with reference to the zero-point of the scale. All this, I repeat, is rather unimportant, though it gives us a first glimpse of phenomenal data which we tend to ignore when we use the apparently simple terms "coincidences" or "pointer-readings" and claim that they are the only perceptual material used in physics.

It is much more important that in physics the pointer-readings are numbers which have various *names*. The name may be "ohms," if we measure electric resistance; "ampères," if we measure the intensity of a current; "degrees," if we measure temperature; "miles per hour," if we look upon the speedometer of our car; "atmospheres," if we measure air-pressure; "grams," if we measure mass—and so on through the whole domain of physics. A mere coin-

cidence as such does not tell us with which of all these possibilities we are dealing in a given case—not even if we include the reference to the scale as a whole when speaking of "the coincidence." Quite apart from such names as indicate the various *units* of measurement, we must evidently have some source of information which tells us whether a coincidence before us means so much of "resistance" or "current" or "temperature" or "velocity" or "pressure" or "mass"—and so on. There may be, it is true, some names on the instruments which indicate what they all are meant to measure. But such percepts again are unimportant and obviously once more mere substitutes for something more essential. Where is this more essential evidence which in each case gives pointer-readings their right names? It cannot come from mere thinking which, as such, would have no reason to give the name of "pressure" in one definite case rather than in any other. No doubt, something in percepts—however much pervaded by thinking—must enable us to apply the right names to all the various coincidences. And at this point it will begin to be much clearer that coincidences as such are by no means *the* phenomenal material of physics. Obviously *two* questions are to be answered, if we wish to make any reasonable use of pointer-readings: first, such words as "resistance," "current," "temperature," "velocity," "pressure," "mass" must have a meaning and, secondly, we must be able to decide which of these various entities we are, in a particular experimental situation, about to measure by a pointer-reading.

The first question brings us back to the problem of definitions in physics. That these are not given by so-called "dimensions" has been shown above. In one of his books Professor Eddington discusses the problem of defini-

tions, and indicates that the meanings of physical terms constitute an endless cycle in which one term refers to others, these again to others and so on, until some step leads back to the point where we happened to ask for the meaning of a first term.[4] Familiar conceptions, he says, have gradually disappeared from the list of physical terms. Their places are now occupied by mere symbols of entities whose nature we do not claim to know. And it is these symbols which, in reference to each other, fill the cycle. One might wish that at this point a more detailed report had been given as to the relation between such symbols and definite observational situations.[5] Whatever the symbols may be when we meet them on pages of the *Philosophical Magazine*, or any other journal of physics, the experimentalist must be able to decide to what concrete data of his perceptible experimental set-up the various symbols refer—and vice versa. Otherwise he could not verify or refute by experimentation what the mathematical physicist predicts in terms of his symbols. It follows that so long as the symbols are of any use in physics their meanings must contain not only phenomenal ingredients in general; rather they must refer to specific aspects of perceptual situations, since it is in these that all symbolic activities of the theorist are eventually tested.[6] This means on the one hand that we must have as many different and characteristic perceptual situations as we

[4] *The Nature of the Physical World*, pp. 247 ff

[5] In *New Pathways in Science* (pp. 102 f.) I find the statement. "We must make sure that the quantities or characters that we speak about are directly or indirectly definable in terms of experience—otherwise our words convey no meaning" This seems to me quite true. But what is the relation of this statement to those to which I refer in the text?

[6] This must be true at least of *several* symbols. I do not overlook the fact that some concepts like "entropy" or "action" refer to perceptual data *via* other concepts for which the reference is direct.

have symbols, the theoretically computed behavior of which is to be examined in the laboratory. On the other hand, I repeat, it means that each of these symbols must by its definition be strictly coördinated with one particular perceptual situation. It can therefore not be a correct thesis that the physicist's symbols are merely defined in a self-sufficient cycle of mere symbols.

The reader will not be uncautious enough to propose that the perceptual data in question are the pointer-readings of experimental physics. This would only lead us back to our original predicament: Pointer-readings as such are the same perceptual data whatever we intend to measure. What we need are *different* perceptual contexts inside of which pointer-readings acquire in each case a particular meaning, "current" now, "pressure" another time, and so on. Once more, beyond mere coincidences which are a common feature of all experimental tests, there must be as many distinguishable phenomenal situations in laboratories as we have symbols to test. And it would help us quite as little if one should say that a first pointer-reading should be interpreted in the light of another pointer-reading. So long as the second is nothing but a mere coincidence precisely as the first, so long as no "qualification" is given to it by a larger perceptual context,—it could throw no light on the first coincidence. No amount of pointer-readings as such could change this state of affairs. Ten blind persons do not see more than a single blind man.

It amounts then to this: All the more basic entities of physics, those which have citizenship both in actual experimentation and in theory, are ultimately defined with reference to concrete perceptual situations—now as they always have been. There is a "current"-situation, a "mass"-

situation, a "temperature"-situation, a "pressure"-situation in *perception*. If these *situations* did not tell us what we are measuring, no pointer-reading would have any sense; and if symbols did not refer to such situations, they would not have the slightest contact with experimental facts—which, of course, are phenomenal facts. A short time ago I distinguished two tasks: The first was to discover the source of the meanings of all the essential terms which we find used by the physicists. The second would be solved, if in experimental situations we could always decide to which of these entities a particular pointer-reading actually belongs. We realize now that in both cases the solution is the same. As in certain perceptual situations the meaning of the term "electric current" is given, so additional data in the same perceptual situations will tell us that the pointer before us indicates by its position "intensity of current." The additional percepts consist in the structure of an apparatus, the galvanometer, and in its perceived connection with the main part of the situation, the circuit.

Not much is changed by the fact that some thinking will have to accompany our perception of a situation if it is to be fully understood. Such thinking remains focussed on the percepts in question and perhaps on ideas which represent further physical situations.—Again, it is hardly essential that a "current"-situation may vary in many perceptual details. In the structure of "current"-situations there must be some *invariants* which characterize such situations independently of all unessential perceptual traits by which they differ. Give a physicist sufficient time, and he will recognize a "current"-situation, even if all the local parts of it are of unusual shape. I admit, he will have to do some thinking. But such thinking can lead to correct

recognition only so long as the fundamental symptoms of a "current"-situation are present, and as the thinking refers to these. It is again to these invariants of certain perceptual situations that the mathematical vector-symbol for "current" must be related. Otherwise, as we have seen, the symbol would be meaningless in empirical science. The same reasoning applies, of course, to other physical concepts.

Symbols are therefore ultimately defined in perceptual contexts, and all the various pointer-readings have their specific meanings in such contexts. For mathematical purposes we may eliminate from these contexts whatever is not strictly essential, until at last the residuum seems hardly to have much affinity with perception. This process is desirable in so far as its result is meant to be of general use apart from all accidental traits in individual cases. Still, for reasons which now ought to be evident, some remnants of structure must be left in which the meanings of the "abstract" symbols agree with perceptual situations. There is almost a humorous touch in the fact that, with all our aversion to mere percepts, eventually the symbol as a rather obvious percept should absorb our attention altogether, and that, by contrast, we should overlook the little delicate spider-web of phenomenal structure which, as its meaning, still adheres to that percept. But it is this bit of phenomenal structure which gives each symbol of the theorist a definite place in the perceptual world of experimentation. In this world corresponding structures appear in more concrete phenomenal "materializations," and thus the right coördinations of symbols and facts become possible.

Eddington speaks of the scheme of relationship, the structure, in which the physicist puts his pointer-readings.

As he uses these terms they refer to general relationships which we call "physical laws." We are at present not concerned with these. Apart from general laws, however, concrete experimental situations are perceptual contexts within which the various pointer-readings have definite places and significance *directly*. Since the *physical* meaning of the pointer-readings is defined in these contexts it follows that, beyond the phenomenal world, *we attribute to the physical world similar structures in which the corresponding physical coincidences occur.* The structure in which we actually arrange the physical coincidences is in its basic scheme given with the pointer-readings in perception. Consequently not only phenomenal coincidences are used in the construction of the physical world; the contexts or structures in which they phenomenally appear are likewise assumed to be common traits of perception and of physical existence. Physical structures in this sense are different from perceptual structures in that they may not "look" or "feel" like perceptual contexts. For this reason and for the sake of accuracy we prefer to describe physical structures mathematically. But in all major respects the same procedure might be applied to perceptual structures. And what I wish to point out is merely that in such a description the most essential traits of experimental or perceptual contexts are the same as those of their physical counterparts. With respect to these traits the perceptual and the physical structures are *isomorphic.* If they were not, we could have no physics.

It seems reasonable to assume that an analysis of "physical laws" as general and abstract structures would lead to about the same result. The terms which we combine in physical laws refer to those entities ("current," "resistance," "magnetic field," etc.) whose meaning is given in definite perceptual situ-

ations. On what basis should the behavior of *particular* physical entities be combined in one particular physical law or "structure," if there were no experiential, i.e., perceptual, context that would ask for just this combination? In actual fact, we do not observe a "resistance"-situation by itself, a "current"-situation by itself, and so on. Rather, one observational context has, in addition to a "current"-aspect, a "resistance"-aspect, a "magnetic" and several other aspects. The theorist will have to admit, I believe, that in the given context these aspects occur in a definite structural relationship, that the quantitative side of this relationship is expressed in a physical law, and that the terms of the law are tacitly conceived to be in that structural connection which we have before us in the experimental situation as a perceptual context. In this sense the structure of the law is not, by pure thought, inexplicably added to an aggregate of mere pointer-readings. With their *observational* structure the pointer-readings are transferred into the scientist's construction of the physical world; and it is this structure which also holds the terms of a physical *law* together.

To abstract from observational physics the essentials or invariants of all the different experimental "situations"—and thus the ultimate meanings of corresponding physical concepts—is hard work, which I leave to the epistemologists of science. Even so we may describe some of the consequences which, for the nature of the physical world, follow from our argument about the pointer-readings. A few examples will make it clearer than general explanations what is meant by a common structure of perceptual and physical situations. And such examples need not be technical.

At first the foreign aspect which Eddington attributes to the physical world may seem to be little changed by the discussion of the last paragraphs. As a matter of fact the change is radical.

Pointer-readings *belong together* in concrete situations. This applies to them as perceptual entities; it also applies to their counterparts, the physical coincidences. Thus, the observations which we make on one thing as a percept are, as a rule, referred to one physical object in the physical world. This is implied in many statements of Eddington's. We have, however, to stress the fact explicitly, since in consequence of it the physical world "of mere coincidences" acquires a fairly familiar aspect. An elephant, for instance,—whom Eddington takes as an example—"is a tube in the four-dimensional world demarcated from the rest of space-time by a more or less abrupt boundary." [7] Under normal conditions the Theory of Relativity may here be neglected, because the elephant is not apt to move past our eyes or other instruments with anything approaching the velocity of light. We are therefore allowed to split the four-dimensional world of space-time into space and time, and to consider the elephant as an entity in space which has a certain fate in time. He appears to us as a three-dimensional percept, segregated from his perceptual background, and, as we learn from Eddington, he is also one segregated object in the physical world. Appropriate pointer-readings would show that the parts of just this object tend to remain together, while the object as a whole may change its spatial relations to other objects much more freely. Should the elephant lose one of his huge tusks, perception would show us two separate things where we had one before. But pointer-readings would reveal that the same is true of the physical elephant and his physical tusk, since from now on these two objects would move freely in relation to each other. In the language of Relativity Theory, the elephant-tube in space-time

[7] *The Nature of the Physical World,* p. 256.

has split into a tube for a less symmetrical elephant and a smaller tube for the lost tusk. The normal elephant-percept has four legs, two tusks, one trunk and one tail as parts which stand out from the more compact remainder, their lengths being considerable in comparison with their diameters. However abstractly we may formulate corresponding data in the physical world, their meaning there is precisely the same. In other words: As to his main structural traits the elephant of physics agrees closely with the elephant of perception. He, too, has four legs, two tusks, one trunk and one tail, even if you speak of them in terms of pointer-readings.[8]

In an elephant there is nothing in particular which would forbid us to generalize these statements. I see three people before me on the street, and three physical people are there on the physical street. Visually each of them has two legs and one head; a spider on the wall eight legs and one head. The structures of pointer-readings which the physicist would transfer to the physical world will in these cases again preserve all such traits without any essential alteration. We need no further examples, of trees, of houses, pieces of furniture, books, pencils and other fa-

[8] Eddington, I must remark here, would not commit himself to the opinion that the *twoness* of two physical objects is "just like" the twoness of two percepts, or, in other words, that integers should be "freely displaced" from the world of experience to that of physical entities (*New Pathways in Science*, 1935, p. 23 f). I cannot say whether the twoness of two physical objects is *just* like that of two percepts It seems to me, however, that integers of the physical world must be like those of the phenomenal world to that extent, that fundamental mathematical operations can be applied to both in the same sense and according to the same rules. Otherwise, *what* meaning would the term integers have in physics? And what meaning would the methods of mathematical physics and many statements of the chemist have? I grant that the use of integers in physics has its limitations and may mislead us in certain cases; but every psychologist knows that the same is true of their application to phenomenal data.

miliar things.[9] For the most part not only their numbers as distinct thing-percepts but also the major characteristics of their structural make-up are the same physically and in perception. Even if there is an occasional exception, as when in dim light we may have one thing-percept where two objects are present physically, what does it matter for our question of resemblance in principle so long as the rule holds in general? [10]

It would not be amiss to say that in *all* these cases it is really structure in which the world of percepts and the physical world have so much in common. Resemblance as to the demarcation of definite objects, and therefore to their *number,* means in fact similarity in the gross structure of the two worlds. And then *inside* such particular objects there is again structural resemblance between the perceptual and the physical world. In this respect, too, the resemblance may go very far indeed. A ring is in perception a figure characterized by the peculiarity that, around something else, it bends back upon itself. If the corresponding physical object is described by the physicist, will he deny this topological fact? No, it belongs to both worlds. Or again: Before me I see a river separating its two banks. Is the physical river not between the two physical banks? It is. This structure or arrangement of the

[9] In the Introduction to *The Nature of the Physical World* Professor Eddington implies most clearly that to the table, the chair and the pen in his perceptual study there correspond a table, a chair and a pen in the physical study But this resemblance between the perceptual and the physical world is not given much attention when in his chapter on pointer-readings the same author represents the physicist's world as a pattern of mere symbols quite unlike the world of percepts.

[10] I should perhaps remark in passing that our argument often applies to *events* as they follow each other in time I count 10 beats of two tuning-forks which have approximately the same frequency of vibration. How many oscillations of intensity would be found in the corresponding stretch of physical waves? Again 10.

world of percepts is, as a structure of pointer-readings, simply preserved in the physical world. Why then should we frighten people by abstract statements about coincidences as the only material common to the physical and the perceptual world? If you do not add that, besides, the concrete structure of the two worlds is nearly the same in many respects, those statements will be seriously misleading. In the world of percepts the scientist leaves his home, walks along some streets and passes through the entrance-door into his observatory. All this can be described in terms of pointer-readings; but, if these pointer-readings are to have any physical significance whatsoever, they must be structurally related. When they are carried over from the phenomenal to the physical world, essential phases of their structural framework accompany them from the perceptual to the other domain. And then we find that the scientist as a physical object moves physically from his physical home through physical streets into his physical observatory, precisely as the scientist-percept does in relation to corresponding thing-percepts. I think it is a consolation.[11]

In some of our examples—the elephant, the people and the spider, the ring—the similarity between physical and perceptual structure approaches an agreement in form. Where the thing-percept has a protruding part the same is true of the physical object. The ring has a hole both phenomenally and physically. Not infrequently such a resemblance approximates the level of quantitative exact-

[11] The reader is once more warned against a mistake which seems to be easily made. It is *not* possible to reduce the structure of pointer-readings or other coincidences to more pointer-readings as such. Even the fact that, in a particular case, two pointer-readings give by their difference a distance in *space*, not a difference in *mass*, can only be recognized by reference to something which is not in itself a pointer-reading.

ness. Before me I have a simple electric condenser which consists of two metal plates. When looking at the instrument from one side I have right before me a perceptual circle which is one of the plates. When measuring the plate I find it to be physically symmetrical around a central point. But this is a structural trait which is also implied in the perceptual circle.—Or again, when changing my position appropriately I discover that as percepts the two plates are plane surfaces parallel to each other. Physical measurement shows that the same is true of the physical plates; or at least it gives me data which are structurally equivalent to such traits of the perceptual situation. Measurement, it is true, goes farther than direct inspection of the percepts. It will reveal slight deviations from those regularities where simple perception reaches its limits of accuracy. Still, up to this point isomorphism is almost complete.

Eddington emphasizes the fact that we can never say what physical things are ultimately made of. If this be granted, it does not follow that, apart from coincidences and abstract laws of their connection, the physical world is hidden to our understanding. As a rule it contains objects—whatever their ultimate nature—where we perceive things. Structurally there is much resemblance between thing-percepts and corresponding physical objects. Such objects are commonly related in physical space more or less as thing-percepts are in phenomenal space. If those physical objects change their locations, the same is generally true of corresponding percepts. I need hardly repeat that the phenomenal and the physical worlds differ widely in other respects. Few qualitative traits of perception, and perhaps no 'subjective' phenomena, are attributed to physical nature. Direct perception may also deceive us

about the quantitative traits of some structures; it will, in this respect and in general, never reach the *accuracy* of indirect measurement. It does not easily reveal most of the electric and magnetic properties of the physical world—which accounts for the fact of their later discovery. It completely hides the atomic sub-structure of physical objects. Nevertheless, in their "macroscopic" structure phenomenal and physical situations resemble each other closely enough.

III

The "macroscopic" or "molar" aspect of the physical world, however, is sometimes regarded almost as an illusion. Modern natural science is atomistic. We find at present most of experimental and mathematical physics concerned with such entities as molecules, ions, atoms, electrons, protons, neutrons, and so on. Knowledge of these particles and of their behavior may easily seem to be the only goal of science. The particles themselves will consequently appear as the only "real" content of nature. Since under normal circumstances perception does not give the slightest indication of their existence, in this respect at least the difference between the phenomenal world and nature is apt to impress everybody. The physicist likes to give this impression a disturbing touch. The pavement, he would say, on which you walk along the street, is for the most part empty space. In so much emptiness tiny electric charges rush about here and there, and it is only the ever-repeated impulses of such charges which keep your feet and all the rest of you "above the ground." Counted in cubic centimeters of stone there are a great many of them—and this fact makes you float along the surface

with a feeling of safety. It remains none the less true that, compared with the empty part of the pavement, those bits of charge fill hardly any space whatsoever. To this extent you are virtually walking on holes. This achievement, it is true, may appear more plausible if you learn that exactly the same reasoning applies to your feet and to your organism in general. As a physical object you are at least as much empty space as the pavement is. Comparatively speaking, it is an enormous distance—through emptiness—from one of the tiniest particles of your organism to the very next. But you are not likely to enjoy this picture of your own looseness any better than that of a pavement composed mainly of nothing.

I do not think that this description of physical objects is quite adequate. Before we criticize it, however, we should perhaps consider to what extent the atomic nature of the physical world agrees or disagrees with earlier arguments of this chapter. According to these arguments the meaning of all concepts which we use in describing physical entities and events is ultimately derived from phenomenal data. Does this hold in the case of atomic entities and their behavior?

Atomistic hypotheses were developed long before any observation could demonstrate the "atomic constitution" of the physical world. It is obvious that in such early assumptions an atom and similar concepts were conceived after the model of larger empirical objects which move freely as wholes, and are not easily divided into parts. It was merely the small size of atoms, their more perfect elasticity and the higher degree of their resistance against destructive forces which supposedly distinguished them from ordinary things. In other words, the atoms of earlier atomistic thinking had no properties the type of which was

not well known from common phenomenal experience. The history of the kinetic theory of gases and of the theory of solutions proves clearly how much could be achieved even with these primitive concepts.

Since then atomism in physics has undergone a fundamental change. However, the decisive characteristic of this change did not at first consist in the development of more refined conceptions of the atom and smaller particles. This development was made possible only by the more fundamental fact that the atomic constitution of the physical world, until then a matter of theory only, became accessible to more direct observation. The paths of *single* particles were, by Wilson's method, rendered visible on the photographic plate. The impulses of *single* atoms—or ions—of helium could be observed and counted as discrete "scintillations" on a surface of appropriately chosen material. From the realm of hypotheses the atomic constitution of nature, the discreteness of particles, was thereby transferred to the field of perceptual facts. It is this progress which immediately affected the knowledge of particles so much that their velocity, their electric charge, their mass, soon became accurately determined data, and that the atom began to reveal its inner structure. Among these new discoveries there were at first only few in which the behavior of particles was found to differ considerably from the previously known behavior of larger objects. Discreteness of the path of one electron, as shown by Wilson's procedure, means essentially the same as discreteness of the movement of one stone or one cloud; as a common electric charge, when moved, constitutes an electric current, so does the quickly moving electron; and as the magnetic field displaces a common current, so does it bend the path of the electron.

Not until quantum physics demonstrated a strange discontinuity in the *behavior* of atomic entities and of light was there any serious suspicion that customary notions did not apply to the remotest properties of nature. Precisely what concepts will eventually prove adequate in the treatment of quantum physics remains to be seen. No physicists will assume that such concepts can wholly correspond to familiar ideas as directly abstracted from the behavior of thing-percepts. Nevertheless, even the most foreign symbolic language in which at present the theorist makes his statements on quantum physics remains physically meaningful only to the extent to which this language points ultimately back to some phenomenal sources and to possible verification in perceptual situations. If some property of the quantum or of photons should prove unintelligible in any phenomenal terms, it would necessarily remain inaccessible forever. As a matter of fact, however, quantum physics tends not toward remoteness from perceptual facts but rather toward the elimination of all concepts with which no observable fact could correspond even under ideal conditions of observation.

The atomic constitution of nature appears in corresponding perception only if the physicist makes use of particular devices which have been invented for the purpose. Direct observation of thing-percepts and their behavior will hardly ever give any evidence of atomic facts. As a rule thing-percepts seem to us "dense," "continuous" and, in general, "quiet," while the physical things in question are said to consist of discrete particles, widely scattered in otherwise empty space and rapidly moving in all possible directions. If this be the truth about physical objects we seem forced to confess that perception is to an amazing degree illusory, giving us as it does the impression of definite continuous

large-scale objects which behave as wholes. It may be a correct statement that all physical entities of which we can know must be akin to some phenomenal data. But with the same basic colors obviously altogether different paintings may be produced; and nature seems patterned after a scheme which does not agree with the pattern of perception. Nature, its entities and the laws of their behavior, seem to have a thoroughly *microscopic* constitution, while thing-percepts and their behavior are essentially *macroscopic.*

Since the physicist himself uses these terms in application not to the atomic constitution of nature on the one hand and to the "molar" pattern of perception on the other, but to two different sides of *nature,* we should not too hastily assume that the term "macroscopic" refers to an illusory subjective impression. Only a few decades ago atomic events became directly accessible to experimental research. It is not surprising that the opportunity of investigating an almost entirely new field should for a time absorb the physicist's interest altogether. Influenced by this attitude of the expert the layman is apt to give less importance to that vast body of physical knowledge which had been built up before the era of atomic research. What was the characteristic of such knowledge, since it could not yet be truly atomistic? It was, as a matter of experimental experience, wholly macroscopic. Atomistic theories existed and were certainly most influential in chemistry and in thermodynamics. The only way of testing their validity, however, consisted in the verification or refutation of such conclusions as followed from the atomistic assumptions for the case of macroscopic observation. All matters of fact were therefore macroscopic facts, and all directly verified laws were macroscopic laws. This means that the physical

objects in question corresponded to macroscopic percepts, and that the laws referred to macroscopic relations between such entities.

This knowledge has certainly not become obsolete and useless, because in the meantime modern tools have transformed atomistic speculation into atomic facts. Ohm's law as applied to stationary currents in metals and electrolytes —a typical macroscopic law—is not wrong because now we know about electrons and ions. In fact, in this particular field observational physics is still about as macroscopic as it was forty years ago. On conductors electric charges still assume a distribution in which electrostatic energy has the smallest possible amount; this law is not changed by the fact that such charges consist of electric particles. Surface tension still gives a liquid surface the smallest possible area whatever the atomic constitution of the liquid may be. And independently of their molecular make-up macroscopic objects swim in water according to Archimedes' principle. Even if all these are old stories for the physicist of our time they are not thereby rendered less true. On the contrary, he will agree that such parts of physical knowledge will probably never be profoundly altered by future research.

Just as these laws are macroscopic laws, the objects to which they refer are macroscopic objects which have a macroscopic volume, a macroscopic shape and macroscopic boundaries. The mercury in a glass tube does not only consist of particles which are, as particles, different from those of the glass; but macroscopic physics considers the behavior of the volume of mercury as physically demarcated from the behavior of the glass—notwithstanding their mutual dynamic relation. The object which swims in water according to Archimedes' principle is regarded

as one thing, the water as another, though at the same time their dynamic relationship is fully realized. In both cases the implication is not only that throughout one such object chemical properties are often approximately the same, but also that the chemically homogeneous material forms a natural unit in physical behavior. It is, however, not necessary that different units or objects should be chemically different in order to remain macroscopically demarcated from each other. In elastic or semi-elastic impact two billiard balls are two definite macroscopic objects with definite boundaries, although both consist of the same material; and in the law of their behavior such demarcation is tacitly implied. Conversely, often one object may consist of several substances.

When a physicist tells us that a macroscopic object is "really" a swarm of particles, do we understand him correctly if we assume that atomic physics does not recognize the existence of definite macroscopic objects? Are macroscopic objects *dissolved* into particles? At first it might appear as though only one aspect, either the microscopic or the macroscopic, could correspond with physical facts. And yet there is no such alternative. Both the demarcation of macroscopic objects and their atomic constitution are legitimate notions which are well founded on physical evidence. Particles do occur as separate entities. They are also "real" parts of macroscopic objects. But functionally they are not quite the same entities in the second as in the first case. In macroscopic objects they mutually influence each others' behavior in such a manner that their individual freedom is very much restricted. Their positions, their displacements and their velocities are no longer altogether independent data; for each particle moves "in the field" of surrounding particles, and vice versa. The forces

which constitute this field are due to the electric charges of the particles. Their general effect is that, under given conditions of temperature, the average distances between the particles tend to remain approximately constant. In liquids this leads to constancy of volume, in solid objects to constancy both of shape and of volume. With a change of temperature the average distance of the particles increases or decreases while the particles themselves move more quickly or more slowly. So long, however, as the object does not in any considerable amount emit single particles in the form of a gas, its volume remains one definite context of particles which are held together by their mutual forces. Even if there is some evaporation or, conversely, absorption and adsorption of gaseous particles, the object has at a given time a fairly well defined boundary within which particles cohere dynamically in one macroscopic context of particular intimacy.

Such a context is no less "real" than are the particles. So far as we can show that inside definite macroscopic volumes particles are held together by specific interaction, and that the boundaries of these volumes are the boundaries of such cohesion, so far the concept of macroscopic objects as physical entities is fully justified. One side of nature would be ignored if our present interest in microscopic physics were to make us overlook the existence of macroscopic contexts of particles. Such contexts are not products of illusion, their boundaries are not arbitrarily selected for subjective reasons; they are given data of the physical world. Thus we have not to choose between the microscopic and macroscopic aspect of nature. Both aspects of the physical world are founded on objective facts; they represent *different levels of physical organization.* Even in microscopic physics we find several levels of such

organization. Most atoms, for example, are highly complicated structures. Nobody denies the existence of their components, electrons and positive nuclei. At the same time the structure, i.e., the atom, is everywhere recognized as a definite entity which cannot be described by a simple enumeration of all its components. In the atom these components are functionally not independent entities, and therefore the atom-unit is certainly not an arbitrary concept.—Again in one molecule, sometimes a few, sometimes hundreds of atoms are held together by their fields. No physicist would say that we have to decide whether we wish to acknowledge either the atoms or the molecule. Both are definite entities, both concepts agree with physical data, two levels of physical organization have clearly to be distinguished in one microscopic object. Similarly, a *macroscopic* object represents merely a product of organization at a still higher level. It contains particles,—molecules, ions and others. Such particles, however, have lost much of their independence. And as inside a molecule an atom is doubtless not quite the same thing as it would be in complete freedom, so at least the *behavior* of a molecule is, inside a macroscopic object, strongly determined in this larger context. We are certainly much better informed about macroscopic objects, now that we have some definite knowledge of their atomic or molecular constitution. One of the main advantages of such knowledge is, however, that at present the coherence, the demarcation and the macroscopic behavior of a definite large-scale object is much better understood than it was before. Obviously, progress in this direction can not mean that atomistic theory of macroscopic objects denies their existence as definite entities.

Although the existence of macroscopic things is a com-

mon feature of perception and of nature, at least the apparent *continuity* of thing-percepts seems illusory when compared with the atomic constitution of corresponding physical objects. I should, however, hesitate to admit even this much. When we hear that the pavement of the street, that our table and that our organism are much more empty space than anything else, the underlying notion is clearly that particles are tiny bits of "matter," that where we have such matter there is "really" something, while between particles in this sense there is not much worth mentioning. One does not see why in popular presentations of physical knowledge such descriptions should still be repeated. Meant to warn the layman against an anthropomorphic conception of physical objects, they are themselves surprisingly anthropomorphic. Where in present physics do we find any convincing evidence for the assumption that "particles" are in fact what the old term seems to indicate, namely extremely small portions of something called "matter" with a measurable diameter and a sharp boundary? Democritus could have such a notion when he thought of atoms as of tiny editions of macroscopic things. But, quite apart from quantum physics and its severe criticism of similar ideas, a physicist can hardly share this view in our times. Is the "field" of an electron less reliable a physical entity than "the electron itself"? And what does it really mean, if, at least by the terms of our language, we still distinguish between the electron and its field? I do not know of any empirical evidence which would support the distinction. It seems to be a remnant of old materialistic conceptions: these conceptions yielded the "particle of matter"; then, after Faraday and Maxwell, the particle became surrounded by a field. And we still trust "the particles themselves" more than their fields, as though—

in the case of the pavement—we could safely tread on particles but should beware of "mere field." As a matter of fact, insofar as "particles" are known to be fields and field-structures they fill the volume of a macroscopic object completely, and to this extent the object is a continuum. It is only as a field-continuum that it coheres. And the support which the pavement gives to our feet is entirely due to this continuum. It will not yield unless much stronger forces than our weight are applied.

If a macroscopic object is functionally or dynamically a continuum—it would crumble at once, if it were not—perception fails to reveal only one important aspect of the thing: This is the irregular vibratory unrest by which the object is constantly pervaded, and which, from the standpoint of physics, is its content of heat-energy. The vibrations as such are microscopic in nature and, being microscopic, they do in fact not appear in common perception. Only special devices, such as those used in the demonstration of Brown's movement, permit us even remotely to approach a direct perception of so much motion.

On quantum physics. The last paragraphs may seem to contradict the point of view of modern physics in one respect. A macroscopic object, I said, is held together by forces; and the same object is a continuum as a field-structure. Both the concept of force and that of field involve the idea of causality. But this idea is, according to some authorities, no longer accepted as valuable in physics; its place has been taken by the concept of probability. At first probability was a partner of causality—in thermodynamics for instance; then causality began to be regarded as a superfluous notion—in quantum physics,—so that at present probability reigns alone and supreme.

It is essential, not only for the argument of this chapter, that such statements be regarded with some suspicion. The most striking argument against causality in physics is contained in Heisenberg's Principle of Indeterminacy. For causality to be strictly applicable to physical events we should be able to state exactly in what conditions a physical entity is found at a given moment, so that, with adequate knowledge of a definite causal law, we might predict in what conditions we shall find the same entity a short time afterwards. Such conditions cannot be accurately observed, however, for reasons which seem to belong to the constitution of the physical world. Observation itself will alter the physical situation, because observation is not possible without a physical interrelation between the entity observed and the observer or his instruments. And since the amount of such interrelation or interference cannot be reduced to less than a definite quantity, a limit is set to the accuracy with which we are allowed to know about actual states of the entity which we observe. Thus the "real" position of an entity and its "real" velocity—or at least one of these two characteristics for a given case—become within certain limits doubtful notions. If, instead, we speak about mere probability-distributions, both for the position and for the velocity, we remain in the realm of observable facts. But when we have come this far we discover that causality has been lost on our way. Causality implies determination and prediction, and in our new physics of probability-distributions there is apparently no determination and no prediction. It seems to follow that there is no causality in such a physics either.

I have intentionally oversimplified this description of the theoretical situation in order to avoid a difficult technical discussion. We have to be specific, however, at least

in so far as the Principle of Indeterminacy has significance for microscopic events only. Electrons and their behavior tend to disappear in a cloud—or in waves—of uncertainty, but not events in macroscopic dimensions. Their physics is not measurably altered by Heisenberg's theorem. Should we conclude, then, that the concept of causality applies to macroscopic physics, while the same notion is valueless in the microscopic world? It seems difficult to hold this view. If there is no causation in microscopic events, a great number of microscopic events would probably have to be described as a "combined" probability-distribution to which the notion of causality could no more apply than it does to one microscopic event. I doubt whether under these circumstances the existence of relatively permanent macroscopic objects could be made plausible. In fact, such a dissolution of the physical world has never been proposed in quantum physics.

We speak of causation when the behavior of an entity, a datum, an "observable" is found to be altered by or to depend on the presence of other entities, data or observables. Special causal laws describe particular cases of such alteration or dependence. More cautious terms than these may be used. In a Positivistic mood we might say, for instance, that causal laws refer to a regular *correlation* of observable facts. It does not matter for our problem. This problem is whether or not causation, as physical dependence or as correlation, is attributed to microscopic events by quantum physics.

It seems fairly obvious that it is. The most popular introduction of Heisenberg's principle starts with the consideration of an electron the position and the momentum (or velocity) of which are to be measured. In order to measure we observe; and in order to observe we expose the

particle to light. If the light is capable of giving an accurate measurement of position, it will by its impact change the momentum of the particle. If, on the other hand, we try to reduce this disturbance, our measurement of position will become correspondingly less accurate. The difficulty is due to the fact that light energy does not occur in amounts which are smaller than Planck's quanta; it is a difficulty in principle. Thus we find that for an electron the concepts of accurate position and of accurate velocity have only a limited meaning. Consequently statistical concepts have to replace the idea of causality.—It can hardly be denied that in this argument, when literally interpreted, the fact of one special causal relation which is involved in any observation is said to destroy causality. We may not be expected to know in a given case precisely *what* change of momentum impinging light will produce in the electron. But on the authority of a famous experiment of Compton's it *is* assumed that impact of light and change of momentum in general are "correlated" physical facts.

To this extent causality is not eliminated by quantum physics. Quantum physics does not rely on probabilities exclusively. It recognizes, even in microscopic events, such connectivity of observables as is implied in the terms "dependence" or "alteration," although it abstains from the notion of complete determination. The same conclusion follows if we consider that the concept of "field" is freely used in the modern theory of microscopic events. As a matter of fact, one of the first tasks which the new theories solved refers to the behavior of electrons and other particles when under the influence of a field of force. The meaning of such terms implies causality in quantum physics. Even if forces or the mutual dependence of phys-

ical entities could not be accurately determined, even if, here again, probabilities would have to replace more definite data, we should still have to distinguish between such probabilities of *dependence* or *correlation,* i.e., of causation, and those probabilities which refer to the correlated entities, their positions and their velocities. In any case quantum theory assumes without hesitation that in the neighborhood of the positively charged nucleus of an atom electrons are deflected towards the nucleus and positive particles away from it. This is causation; and it remains causation, even though the electrons and positive particles are not in every respect strictly describable entities. Quite apart from probabilities, the positive nucleus has one influence on the negative electron and the opposite influence on a positive particle. Since the status of neither is clearly defined before the causal event, the same will be true during this event itself. Consequently quantum physics shows not how individual particles, but how the probability-distribution of particles is affected by their passing through the field of the nucleus.

The new development will probably lead to a reformulation of the principle of causality. But even at the present time it can safely be said that this principle will by no means be eliminated. In a way *determinism* seems to have failed. But determinism in this sense is the assumption that the description of causal relations can be differentiated *ad libitum,* down to any arbitrarily chosen degree of detail. Even if there is something in nature—the quantum—which makes this a meaningless proposition, we cannot for this reason conclude that *all* "determination" of the behavior of one physical entity by the presence of another is a myth. Determination or causation in general pervades the system of physics now as it did before the time of

quantum physics. And this is true at the microscopic as well as at the macroscopic level of physical events.—

It is time to look back. A short discussion of quantum physics became necessary in connection with the concept of macroscopic physical objects. Such objects cannot be defined and described without reference to causal relations. We had to show, therefore, that, contrary to superficial appearance, causality is—with certain restrictions—still a valid notion in modern science. Thus the outcome of our discussion validates the concept of macroscopic objects at the same time.

To look still farther back: We were interested in macroscopic objects for the following reason. Physics, it was stated, proceeds on the assumption that certain structural traits of percepts agree with the structure of corresponding physical situations. It is, however, only macroscopic structures which can be common characteristics of the perceptual and the physical world. And this statement has sense only if the notion of macroscopic objects is found to refer to definite physical entities. We have, I believe, been able to show that it does. It is therefore a meaningful thesis that perceptual and physical contexts are isomorphic in essential macroscopic respects, and that to this extent there is resemblance between the phenomenal and the physical world. In the next chapter it will be shown why this is relevant insight for the main purpose of our investigation.

CHAPTER VI

On Isomorphism

I

In many branches of philosophy we have to overcome extreme intellectual difficulties; but in the following discussion the obstacles will belong rather to the emotional sphere. I propose to consider the nature of cortical processes; and many philosophers dislike to hear much about the brain when philosophical problems are being discussed.

To some extent this aversion has historical reasons. Philosophy still remembers the time when, after the fall of Hegelianism in Germany, the pendulum began to swing toward the other side, and Materialistic notions became quite popular for a while. It was the principal mistake of this movement that it represented mental life as the product of something altogether different and, doubtless, functionally inferior. For in those years crude ideas about matter and its behavior predominated in science. Still cruder ideas were held by those who in their Materialistic doctrines tried to apply science where, with its insufficient knowledge, it could not really be applied. On the other hand, some of the essential characteristics of mental life were well known to philosophers. For this purpose no special research is needed. The very simplest observation will find some structural traits of mental operations for which there are no analogues in the realm of "moving matter." The statement, therefore, that mental

life is a product of matter and its motions, could hardly have a very clear meaning. Consequently the philosopher might well have shrugged his shoulders, if it had not been for one reason: Whoever made such statements must either be blind to the essential characteristics of mental life, or else he had to interpret them as mere illusions. Supposing now that for the philosopher the very essence of human nature and value is expressed in such traits of mental activity, we must expect him to regard Materialism as a serious danger. So, as a matter of fact, he did, and most philosophers still do so.

But they do more. Concepts and words are in one respect similar to sponges. If for generations an object has been highly valued, the corresponding concept and even its verbal symbol will eventually be thoroughly impregnated with dignity—precisely as a sponge absorbs a liquid by which it is surrounded. In a case of aversion both the concept and its name will soon be charged with negative qualities. While it is not easy to dry a wet sponge entirely and quickly, it sometimes seems almost impossible to remove from a concept those value-qualities which it has once absorbed.—I am not in the least disturbed by the fact that the term "Materialism" will not easily lose its unpleasant sound. But it disturbs all serious discussion of several problems in philosophy that such terms as "organism," "body" and "brain" should share the same fate. When Materialism became a concept with dangerous implications these entities acquired a negative character at the same time; because it was in terms of them that the hated doctrine was enunciated, and it was their apparently inferior characteristics to which the Materialist tried to reduce the valuable properties of mental life. These sponges are still wet. It will doubtless take many years before in

philosophy we are all able to look upon the organism and the brain with quiet impartiality, or even with the intense interest which is due to them. Meanwhile their appearance in a philosophical discussion will be resented by many, unless we hasten to give assurances that there will be a happy end; that eventually the merely secondary importance of biological facts for mental life will be consolingly demonstrated. But how is one to give such assurance in the face of overpowering evidence to the contrary?

Long before the middle of the last century there had been other waves of Materialistic thinking. The intellectual level of such earlier theories was, in general, not higher than that of Materialism around 1850. For the reason which I have just indicated, however, the reaction of other philosophers had been no less bitter on those earlier occasions than it was in the 19th century. The aversion to a frank and positive discussion of biological facts in their connection with psychological data has therefore a long history which naturally makes such a dislike even more intense and less rational.

But I do not believe that Materialistic doctrines are the only cause of this attitude. Up to the present time, we have seen, most people have failed to distinguish between the 'body,' which is a percept, and the organism, which is a transphenomenal entity. In the remote past this distinction could not have been made by anybody; at that time the body-percept was quite generally identified with the organism as a physical object. Such a confusion could not fail to have far-reaching effects; for unfortunately the 'body' will often appear as a burden to those who hold particularly high ideals of ethical conduct or intellectual achievement.

At this point we have to introduce a new distinction:

Although in common life we frequently speak of the 'body' as 'I,' words like 'I' and 'myself' have still another meaning in which they refer to a less tangible center of human activities. 'I' taken in this sense may, for instance, concentrate upon 'my left hand' or on some other part of my 'body.' The very words of this sentence prove that, here, 'I' and 'my body' are not the same entity. Whether or not the 'self' in this new sense is phenomenally localized within the body-percept, it seems at an early date to have been regarded as a different entity and as more really the 'I.'[1] It plans what should be done, it chooses between various forms of conduct, it tries to live up to its own demands, it makes efforts to understand where at first a situation lacks clearness. When, upon a resolve of the 'I,' overt activity is needed, the 'body' often behaves like a tactful servant, who anticipates a formal order by appropriate activity. It moves at once and with ease. This, however, is by no means always the case. Much of the resistance against the 'self's' intentions and commands is definitely localized in the 'body.' From a phenomenological point of view there has always been sufficient reason why many philosophers and religious leaders should attribute temptation and passion, whatever disturbs superior purposes, not to the 'self' in this sense, but to the 'body.' Reasonable planning, moral effort and clear thinking as such are hardly ever felt to concern the 'body'; temptation, however, is often enough well localized, and passion tends to penetrate the 'body's' whole frame. Even if the resistance which the 'body' offers is sometimes more passive, it may still be exasperating enough. There are days when even the strong-

[1] With Professor Claparède I find that, as a rule, it *is* localized within the 'body,' even in a definite part of it. *Cf* Ed. Claparède, *Note sur la localisation du Moi Arch. de Psychol. 19* (1924).

est effort will not lead to any concentrated and successful thinking. The 'body' feels heavy, and so long as it does the philosopher seems almost to drag its inert weight along the path of meditation.

Trivial though these observations are, it is for an obvious reason that they are here repeated. For several thousand years they have, with all their implications, been referred to an entity which was body-percept and organism in one. This was true in Plato's time when that philosopher and certain religious groups regarded the 'body' as the prison of the soul; it was equally true of views which were held by more powerful religious movements some centuries after Plato; and it still applies to those philosophers for whom even at the present time the 'body' and the organism are the same thing. Whenever they discuss the relations between mental life and organic events, it is really the body-percept and its behavior which mainly determines the trend of their arguments. The brain, for example, is in such discussions a visual thing which looks none too pleasant; it also is a slippery thing which we do not like to touch. 'Slippery' is a word which here has a phenomenal meaning. It denotes an Ehrenfels-quality in the field of touch. No less a percept, i.e., a phenomenal entity, is the 'body' as a whole. When 'heaviness' irritates the thinker it dwells in this percept; it is *its* 'heaviness.' And it is again this percept in which now and then disturbing urges may be felt. Put in their right places all these experiences lead to fascinating questions about the constitution of the body-percept and about its fluctuating states. It should be obvious, however, that none of these observations is directly relevant to an investigation which aims at an understanding of mental life in its relation to the life of the *organism* as a transphenomenal, a physical entity. It is,

indeed, an essential phenomenological task to examine how the 'self'—in the narrower sense—and the 'body' are related. But it is quite another task to investigate how *all* phenomena, including the 'self' and the 'body,' are correlated with processes in the organism. It would probably mean a real progress in philosophical discussion if, along with the distinction between 'body' and organism, we could make it a rule that the organism as well as its functional relations with mental life be impartially considered, regardless in principle of such shortcomings as we may find in something else, namely the body-percept.

From percepts, it is true, the physicist draws his conclusions about corresponding transphenomenal objects; and when a patient complains about pain in his 'body,' something is usually wrong with the organism as a physical entity. Similarly, the heaviness of the 'body' which disturbs the thinker will generally mean an irregularity in organic function. Whenever the organism is out of order the body-percept seems to indicate the trouble readily enough. We also know certain states of 'bodily' well-feeling which may be a translation, in phenomenal language, of optimal physical conditions in the organism. But there are other things about which we learn little, if anything, from the body-percept. Successful thinking, energetic resolves, will often absorb the 'self' so completely that it almost forgets the existence of the 'body.' Forgotten or not, the 'body' seems to have no major rôle in those achievements. For this reason people will naturally conclude that higher mental operations *succeed* independently of the 'body.' The body seems capable of *disturbing;* but so far as positive achievements of mental life are concerned it tends to become an indifferent neighbor of the 'self.' There is a good phenomenological foundation for this

view; for the *'body'* it may be literally true. Since, however, the 'body' is not being clearly distinguished from the organism, correct observation will in this case lead to wrong conclusions. Between the 'body' and higher mental functions relations are either experienced as unsatisfactory, or they appear as neutral with a minimum of phenomenal reality; seldom are they intense and satisfactory at the same time. There are, I believe, few phenomenal facts which have influenced philosophical thinking as strongly as has this asymmetrical relationship. For so long as the 'body' is identified with the organism, the same asymmetry will, of course, be ascribed to the relations which prevail between the *organism* and mental processes. There will always be a tendency to charge the organism with any difficulties which we meet during our best mental operations. When we are successful, however, our achievements will be attributed to 'ourselves' only.

As soon as we realize that the 'body' and the organism are two different entities, this simple distribution of merits can no longer be justified. The fact may not show itself in corresponding *'bodily'* phenomena, but it is nevertheless true that normal and successful thinking or planning presupposes just as much normal functioning of the brain as certain deviations of thinking are due, for instance, to a paralytic brain. We have no reason whatsoever to assume that the same asymmetrical relation obtains between the various states of the *organism* and higher mental life as impresses us so much when we consider the rôle of the *'body'* during those mental operations. In identifying the 'body' with the organism we are necessarily led to a biased view of all facts which concern the psychophysical problem. The less we hear of any influence which the organism may have on higher mental processes, the better we are

pleased; because the organism is the 'body,' and the 'body' is bad. Once, however, the distinction between the 'body' and the organism is clearly made, no reason is left for this prejudiced attitude. The organism is precisely as "good" when its processes support our highest mental efforts as it is "bad" when in another case its state does not allow of such activities. The 'body' is here no reliable witness. It tends to speak only when something can be said *against* the defendant.

Our present situation is this: We know very much more now about the definite connections between mental life and neural activity than any Materialist around 1850 could possibly have foreseen. Philosophy has not tended to take this change into account. It is as though negative arguments, which eighty years ago were good enough to refute Materialistic speculation, were still sufficient in a discussion of present neurological knowledge and of its relation to mental facts. It remains true that "movements of matter" will never "explain" any phenomenal datum. But is there nothing implied by the fact that, whether they are movements of matter or not, neural events show so much intimacy with all phases of mental life? This, one should think, is a challenging, a most alluring situation for philosophy, which is here confronted by one of the greatest problems of the world. But the organism is still very much the 'body.' Thus, while neurologists and physiologists from their side enlarge our positive knowledge, in philosophy on the other side we generally find an attitude of somewhat passive disdain or of mere defence.

In the meantime those ideas about physical nature which once were dominant among the physicists, and in still cruder form among the Materialists, have been thoroughly and for ever discarded. This is another essential

change in the situation. The physical world with which neurologists find mental life so intimately connected can no longer be adequately described as "moving matter." New ideas have gradually replaced such older notions. If now, from a changed point of view, it should become plausible that brain-events have much in common with essential aspects of the phenomenal world,—what would the consequence be? Many are likely to say that now at last Materialism has become a danger. The more akin nature is to mental life, the greater the chance for Materialism.

I could not share this opinion. Intimacy of mental life and brain-function would disturb me so long as brain-function must be regarded as foreign to my mental operations and still as practically determining such activities. I should fail to understand the relationship and, besides, I should regard it as oppressive. If, instead, it were found that in certain major respects the same happens "on the other side" as happens mentally "on this side," I should certainly feel a great relief. Whatever else the intimate relationship between cortical events and phenomena might mean, it would no longer imply that the course of my mental processes is secretly determined by the principles of an altogether different world.

The other view would be the most remarkable example of what I tried to indicate when I mentioned the outstanding characteristic of sponges. If we could show that cortical processes share some of the main structural aspects of phenomenal experience, they would to that extent become equivalent to such phases of the phenomenal realm; they would therefore be exactly as "good" as are such mental facts; and thus an attitude of resentment would become simply unreasonable.

This may be clear. But I wonder whether it will help. Such terms as "matter" and "brain" are too well impregnated. In philosophy we seem to dislike their very sound, whatever may be the actual nature of physical reality in general and of cortical processes in particular.

II

The main argument of the last chapter applies directly to biology. If anatomists, physiologists and neurologists have any knowledge of the organism and its functions, this knowledge can only be formulated in terms the ultimate source of which is phenomenal. It follows that between biological facts on the one hand and at least some aspects of the phenomenal world on the other there must be resemblance. It is not only the argument, to be sure, which applies to both the physical and the organic realm, but also its limitations. First of all, the conclusion cannot be inverted. Some characteristics of the phenomenal world, for instance the secondary qualities, have no place in physics; they have no place in biology either. Moreover, if certain processes in the human brain are said to be the cortical correlates of phenomenal colors, it is not implied that in those processes themselves there is anything like such colors. According to our present views this holds for all sensory qualities without exception. They all have cortical correlates; but their own existence seems to be restricted to the phenomenal world.

The cortical correlates of mental life or, as we may also call them, the psychophysical processes are more interesting for our purposes than any other biological facts. Requiredness is an aspect of many phenomenal contexts. Like other phenomena such contexts have their cortical cor-

relates; requiredness as one of their characteristics ought to be psychophysically represented just as are the contexts in which it occurs. What is this psychophysical representation of requiredness?—*A priori* a psychophysical correlate and its phenomenal partner might have much or very little in common. One possibility, the most radical, is that, where requiredness occurs in an experience, the corresponding cortical correlate has the same characteristic.

The present question goes far beyond the problems of the last chapter. All biological facts which the human mind can approach may necessarily have *some* resemblance to phenomenal data. The same may be true of all psychophysical processes. It does not follow that the correlate of a particular experience has the same specific characteristics as has the experience itself. Just as the cortical correlate of the color blue is not blue, so requiredness as an experience may be accompanied by processes which are "mere" facts, and are to that extent devoid of any requiredness whatsoever. Such "mere" facts may have to be described in terms the meaning of which is ultimately phenomenal. Nevertheless requiredness need not belong to the terms which this description would use.

Quite apart from requiredness, philosophers assume for the most part that between individual human experiences and their cortical correlates there is no more kinship than is found between the same experiences and, say, the facts of gravitation. One reason for this belief seems to be a certain bias in the choice of those experiences which people compare with brain-events. Our attention is naturally directed toward our 'environment,' and under the influence of sense-physiology we are inclined to regard the sensory qualities as the principal material of which this environment is made. Since the scientist denies expressly

that these qualities have any existence in nature, brain-processes included, there seems to be no further question: the properties of cortical correlates differ widely from those of their phenomenal partners. Thus we forget to consider other attributes of experience. What may be granted in the case of sensory *qualia* need not apply to all aspects of objective phenomena, nor is it necessarily true of mental activities.

It might be objected that, because of their elementary nature, the sensory qualities are happily chosen for a first comparison between experience and psychophysical processes. If no resemblance can be found even in this case between phenomena and their cortical partners, what hope remains of discovering such a resemblance in instances of more complicated and distinguished experiences?—It seems to me that this objection has no force. It presupposes the customary notion that physical reality, both inanimate and organic, represents a lower form of existence. With this premise the highest achievements of nature might conceivably approximate the most primitive contents of human experience. If even on this level comparison fails to show any resemblance between phenomena and their correlates no further investigation seems to be needed. And yet, there might be resemblance between central features of mental life and their psychophysical counterparts even though phenomena on a lower level show no similarity with their cortical correlates.

Quite another reason for the same negative attitude is given in the following well-known argument: Even if we knew as much about brain-processes as the astronomer knows about the movements of stars, would such knowledge be of any help in psychology? It would not. Supposing even that every detail of a person's cortical activity could

be observed, should we know what he perceives, feels and thinks at the time of such observation? We should not. Our observation would deal with data of a foreign world. No psychological interpretation could be given to these data until the man begins to speak and thus to reveal what familiar mental phenomena are accompanied by those strange performances of atoms and molecules in his brain.

The general conviction that human experience and psychophysical events have little in common could hardly be more strikingly expressed. For if in any respect the cortical correlates of mental life were like images of their phenomenal partners, mere description of the first would to some degree be a correct report about the second. It is this possibility which we find emphatically denied in the argument.

The argument betrays its own weakness, however, showing clearly, as it does, that it rests on a special premise. If brain-events are said to constitute a "foreign" world in comparison with human experience, there is invariably an allusion to particles and their behavior as being responsible for the foreign character of cortical processes. In other words, it is tacitly assumed that only *microscopic* events in the cortex can be the correlates of mental life. Behind this assumption there is the other that *real* truth about any facts of nature is truth in terms of particles and microscopic events. Thus it comes to pass that in comparing phenomena and brain-events we concentrate not only on the wrong phenomena but also on the wrong phase of cortical activity.

If all physical reality were actually microscopic reality, the present argument would probably be conclusive. The nature of thing-percepts, we had to admit, could not make anybody suspect that the constitution of physical objects

is atomic. Neither percepts nor any other phenomena, we may now add, indicate by their properties that the brain-tissue consists of particles, and that to this extent psychophysical processes are atomic events. Even the existence of discrete neural entities which are incomparably larger than ions and molecules could not be inferred from the nature of phenomenal facts. We learn in brain-anatomy that cortical tissue contains cells, neurons with their axons, cell-bodies and dendrites; but I do not know of any psychological observation which would hint at the presence of such histological elements in the brain or at the relevance of their function for experience.

In the last chapter we have, however, seen that the microscopic consideration of nature is by no means the only admissible approach to physical reality. Macroscopic physics is at present less popular than microscopic physics; but it will not always remain so. Moreover, the terms "microscopic" and "macroscopic" do not merely refer to a difference between two points of view, one of which comes nearer the truth than the other. Rather the objects of scientific investigation themselves generally have both macroscopic and microscopic properties. Whoever overlooks one of them will therefore miss one side of his subject matter; and this is just as true of macroscopic facts as it is of microscopic data. In our previous discussion we were mainly concerned with macroscopic physical *objects*. These we found to represent dynamic organization on a higher level than that of microscopic entities such as, for example, atoms and molecules. For our present purpose it is more essential that the same distinction applies to the *processes* and *states* through which physical systems pass. Not only are there two ways, the microscopic and the macroscopic, in which such processes may be considered. The processes

themselves have frequently both microscopic and macroscopic properties, neither of which are less "real" than the others. We are seldom justified in considering the processes and states of larger systems as though they were mere aggregates of microscopic events. Macroscopic states, which represent a higher level of organization, do not permit of such treatment; they have to be investigated for their own sake and from a macroscopic point of view, because their own nature is macroscopic.

A few examples will make it clearer what we mean when we speak about macroscopically organized physical states. The facts as such are extremely simple and familiar; it is only their general significance which many fail to realize. Suppose that a steady current of water streams through a pipe, that at some point the main pipe divides into a number of branches, and that farther ahead these branches unite again into one pipe, through which the water is led to its final destination. To superficial inspection it might appear as though the behavior of the water in each branch were merely a local affair, independent of any conditions outside this particular pipe. That this appearance is deceptive can, however, be easily discovered, if in each branch there is a device, a key, by which the current in this branch may be in part or totally blocked from the outside. As soon as this is done in one or several branches the current will flow faster in those which have not been interfered with. Conversely, as everyone knows from his own experience, if water is given one more outlet from a system of pipes, the current in other parts of the system will stream more slowly.—The example is trivial; but even so it contains the principle of macroscopic dynamics. An observer who considers the current in one branch by itself assumes a *relatively* microscopic attitude. Not even the

local current in this branch, however, can be fully understood as a physical fact unless the observer decides to turn his eyes upon a larger context. The branch in question may not exhibit any condition which would produce a gradient of pressure and a current in one direction rather than in the other. If, therefore, the water streams persistently toward one side, it must be conditioned to do so by circumstances outside the branch, i.e., outside the range of the microscopic observer. The same applies to the dynamic interrelation which obtains among the branch-currents. Whatever happens to one of them alters the pressure and its gradient everywhere in the system, so that the rate of current changes accordingly in all the other branches. It follows that, even in the absence of such interference, the water in any of the pipes will flow steadily and at the observed speed only, if it streams at corresponding rates in the others—and vice versa. There is only one distribution of current in which such dynamic interdependence throughout the system preserves the steady state of streaming; and from any initial situation this final distribution develops quickly. It is a self-distribution of current adapted to given outer forces and to the properties of given channels. This distribution is *steady* only as a macroscopic dynamic context.

As the physicist uses the term "microscopic," it refers to events on a *molecular* level. In his terminology the current in one branch of the system is a *macroscopic* part of the total macroscopic distribution. The difference in expression is wholly unimportant for our purpose. We could, instead of a branch-current, consider the behavior of some molecules somewhere in the current. In this case the discussion would have precisely the same result, although we should discover that irregular thermic vibrations of molecules are superimposed on their flow as it is determined in the larger context.

Both the molecules and the branch-current behave in a manner which we cannot understand, unless we realize the larger functional context within which this behavior is determined. If I have slightly changed the meaning of the term "microscopic," the new meaning is, I believe, more valuable and significant than the usual connotation. It seems to me less important that microscopic observation is occupied with atomic entities than that it concentrates on events which do not as a rule occur independently.

Similar examples in more interesting parts of physics are always handled, at least by the scientist, in a manner which implies full acknowledgement of everything that has just been said. The characteristics of macroscopic dynamic states are, however, seldom formulated in explicit general statements. If they were, the discussion of macroscopic in its relation to microscopic physics would probably be clearer than it often is. Again, it could not happen that the same facts, which are simple and almost trivial when we consider a concrete instance, appear strange and slightly disturbing when expressed in general terms. We are not accustomed to these terms. It is the analytical trend of modern science with which the existence of specific macroscopic realities seems to be at odds. And yet there is no objection to an analysis of macroscopic states, provided that we realize what such an analysis can achieve. It can tell us how local events behave within a given macroscopic context, what happens locally as a dependent part of this context or—this seems to be the best expression—how the context behaves at its several points. More often, it is true, analysis is expected to give us *independent* elementary facts, the mere synthesis of which would yield the complex entities found in primary observation. I admit that in this radical sense analysis is not really applicable to macroscopic states; it is incompatible with their nature.

In this field the difficulty is not to *find,* but to *select* examples, because there are so many. Owing to its technical importance the distribution of an electric current has become a familiar instance. From a battery or another source the current spreads through a network of wires and then returns to the source, the seat of the electromotive force which maintains the electric flow. As soon as the circuit is closed the electrons begin to travel in the wires. Any of these wires is capable of conducting current in many different intensities; but after a tiny fraction of a second each wire conducts that one particular rate of current which is compatible with a steady and unchanging flow everywhere in the system. Again we have a macroscopic dynamic state the characteristic self-distribution of which maintains itself only as a whole. If we interfere at one point, we disturb this state all over the circuit and its branches. A long time ago Kirchhoff stated the mathematical rules which the self-distribution of current follows. In the form of his equations the same meaning is implicit which I have here tried to express explicitly in the terms of common language.

I shall not add examples from such domains of physics as electrostatics, magnetization, elasticity, heat-conduction and diffusion. The principle remains essentially the same as in our two instances, whatever the forces and the material which assume a particular macroscopic distribution in each case. It should be realized, however, that the flow of water in a system of pipes and the steady state of electric current in wires agree in one respect which gives both cases a particularly simple character: The channels through which either the currents of water or those of electrons pass are narrow, and they unite only at certain points. This special circumstance makes it an easy task

of elementary mathematics to calculate what the rate of current must be in each branch when the flow is steady throughout the system. In the general problem of self-distribution or of macroscopic states such simplifying restrictions are absent. Suppose that a hollow vessel be given with one opening for the entrance and another for the release of water. The vessel will remain filled if just as much water as enters leaves through the second opening. How will the current be distributed in this stationary state? For each given shape of the interior and for each position of the openings the "lines of flow"—as individual lines, of course, mere constructions of mathematical imagination—will assume a different distribution. Here they will be parallel, there they will converge, there they will bend; and the speed of flow will vary correspondingly. In this case the flow at every point depends even more directly on the current everywhere else, and vice versa, because the system is no longer divided into a number of discrete narrow branches which are in touch only at certain points. A steady state is again established, with this distinguishing feature, however, that in the continuous volume of current the *direction* of flow at each point is as much a problem as is its intensity or rate.

Self-distribution of electric current may occur under similarly generalized conditions, i.e., in a continuous volume. Depending on the shape of its sources and the properties of the medium, such a current will in each case assume a steady spatial distribution in which again both the local *direction* and the local *intensity* of flow are only maintained as dependent parts of the complete macroscopic process. We shall presently return to this case.[2]

[2] For our present purpose we need not discuss the *genesis* of steady distributions. Such a discussion will, however, be of paramount importance in connection with certain other problems of psychophysics.

Experimental physics is not particularly interested in the study of such continuous macroscopic states. As the conditions under which self-distribution develops may be varied freely, an infinite number of macroscopic states is possible in each class: the hydrodynamic, the electric, and so on. The investigation of a number of individual cases would add little to our knowledge of basic physical facts. Besides, what could the experimentalist do? In order to know the distribution of a steady current inside a given volume he would have to measure the rate and the direction of flow at as many points as possible—a thoroughly tedious occupation. At the same time this task would be awkward enough, since, at least in many cases, the very attempt to measure local flow will lead to interference with the distribution itself: The approach and the insertion of a measuring device would generally mean the introduction of new conditions to which the macroscopic state can respond only by a change of distribution. Satisfied that no essentially new facts are to be discovered in this field, the physicist will moreover give little time to macroscopic states in his *teaching*. This is why one can learn a good deal about practical physics without ever hearing very much about this section of science. As a matter of fact, the investigation of self-distribution in continuous media has become a task for mathematicians rather than for physicists. The general rule which macroscopic states must fulfill is easily formulated in mathematical terms. A single differential equation, named after Laplace, will apply to most cases. Unfortunately, however, this equation does not express much more than that in a steady state the forces and the flow at each point should not alter this steady state. Just what distribution would, as a whole, correspond with this condition in a given case is the ques-

tion which the mathematician tries to answer. No direct and simple mathematical procedure is available for this purpose. During the 19th century the invention of solutions even for comparatively simple cases occupied some of the best mathematical minds. The Dirichlet problem and the Neumann problem, formulations of this mathematical task for two slightly different sets of conditions, are noted for their tremendous intrinsic difficulty. In his great treatise on "Electricity and Magnetism" Maxwell has given a most vivid description of the puzzling intellectual situation in which the scientist finds himself when he works in this field. This is not a branch of physics with which other men of science, philosophers and the public will become familiar through popular books. If they did, the belief would not be so general that physics is under all circumstances an "analytical" science in which the properties of more complex extended facts are deduced from the properties of independent local elements. The thesis that analysis, at least in *this* sense, does not apply to macroscopic dynamic states is borne out by the predicament of mathematicians who must find the steady distribution as a whole if they are to tell us what the steady flow is in a part of the system.

"There is one ideal of survey which would look into each minute compartment of space in turn to see what it may contain and so make what it would regard as a complete inventory of the world. But this misses any world-features which are not located in minute compartments." [3]

I do not know of any physical entities to which these words of Eddington would better apply than they do to dynamic steady states which originate and maintain

[3] A. Eddington, *The Nature of the Physical World*, p. 103

themselves in macroscopic self-distribution. As early as 1910 Planck made a similar remark.[4] It seems significant that both he and Eddington were led to such observations while discussing the Second Law of Thermodynamics. This principle always deals with questions of self-distribution; it is besides indispensable to an understanding of the origin of any steady state whatsoever. As it happened, neither the physicist nor the astronomer pointed to those more strictly "dynamic" examples which we have here discussed.

III

If the study of macroscopic steady states as such had ever become an outstanding occupation of physics, the scientific world would now be more clearly conscious of their functional peculiarities. In consequence of it an application of such knowledge to problems of biology might at present be a well established practice. We cannot take it for granted that the scale of relative significance in which the physicist ranks his several problems and subjects will always agree with their evaluation for purposes of biology and psychology. For biology and psychology macroscopic dynamic states seem to be of paramount importance.[5]

Biologists, it is true, regard it as an obvious fact that, quite apart from special processes in its interior, the organism in its totality represents a steady physical state rather than a macroscopic physical object. Its material is

[4] M Planck, *Acht Vorlesungen uber theoretische Physik* (1910), pp. 96 f.

[5] The reader will find a more general discussion of macroscopic dynamic states in the author's book *Die physischen Gestalten in Ruhe und im stationaren Zustand* 1920. (In details, this investigation no longer corresponds to our present knowledge of certain facts in physics and electrophysiology. But its main content is, I believe, still correct.)

not permanent. It breaks down and is eliminated continually, while new material is absorbed from the outside, is chemically reorganized and distributed approximately in the same pattern and at the same rate. It would be a great help if a clear logic or even merely a clear terminology had been worked out for those elementary cases of self-distribution which physics has to deal with. Gradually such a logic might now grow and differentiate, until it became applicable to more and more complex organic facts. As the situation actually is, we simply feel bewildered by the extreme heterogeneity of this unique macroscopic process, the organism. In the treatment of some particular organic functions we are apparently a trifle better off. What happens in the circulatory system approaches so closely inanimate hydrodynamic models that at least *some* apposite concepts of physics can, in spite of a host of complications, simply be transferred to the behavior of blood and lymph. More recently quite a number of special steady states have been investigated within the organism, and Professor W. B. Cannon's fascinating summary of this work, to which he himself has so eminently contributed, is widely known.[6] As yet, however, the connection of the biological facts in question with general dynamic principles of physics has not been clearly stated.

It is perhaps more surprising that in the theory of nervous functions and, more particularly, of processes in ganglionic layers the concept of macroscopic dynamic contexts is so rarely used. Specific order of function in this field has over and over again been explained by a *separation* of elementary processes from each other rather than by the behavior of coherent macroscopic states. McDougall's theory of "drainage" may be mentioned as

[6] W. B. Cannon, *The Wisdom of the Body.* (1932).

an exception; and occasionally another author ventures a slightly heretical statement. Even in Sherrington's work,[7] however, a pattern of activity in the spinal cord may mean either a mosaic of independent local facts or a macroscopic context of function: for reasons which I need not repeat, the alternative does not always seem to be clearly seen. Because of such unclarities as to the very principles of function the more conservative view naturally dominates in the practice of actual scientific work.

The properties of brain-tissue as a physical and a chemical medium are such that in this part of the organism macroscopic contexts of function are not merely possible; it is difficult even to imagine how their occurrence could be prevented. As to the microscopic content of the tissue, which in the physicist's sense are the ions, electrons, etc., such entities move in the brain as near to each other as they do in extraorganic solutions of moderate density. An individual particle will therefore hardly ever be outside the "field" of its neighbors. This makes the tissue as such a macroscopically coherent entity through which "field" extends as a continuum,[8] and, if no special devices or unknown hidden arrangements prevent it, states and processes in this medium will certainly assume the form of continuous macroscopic contexts.

Under these circumstances the argument is not conclusive that, because the constitution of brain-matter is atomic and because brain-processes are microscopic events, there can be no resemblance between the cortical correlates of experience and experience itself. Even if in comparison with phenomena the *microscopic* phase of psy-

[7] C. S. Sherrington, *The Integrative Action of the Nervous System.* (1906).

[8] *Cf.* ch. 5, pp. 178 f.

chophysical processes appears as "a foreign world," can we draw any conclusion as long as experience has not been compared with *macroscopic* cortical states?

To pass judgments on the possibility of macroscopic states in ganglionic layers is, one might say, not the task of physics, but of physiology and neurology. And from this side many would expect the objection that nerve processes are conducted in elementary structures, the neurons, that the functionally essential part of cortical tissue, too, consists of individual neurons, and that thus psychophysical processes must be composed of independent events. I wonder whether our thinking in this field is quite adapted to a new situation which has arisen as a result of more recent discoveries in nerve-physiology. When *we* wish a current to pass along a conductor we choose conditions in such a manner that the current flows in the interior, in other words, that it passes wholly through the cross-section of the conductor. With a natural anthropomorphism earlier theory held a similar view of nerve-conduction. According to present knowledge, however, the propagation of nerve impulses follows a different principle. A nerve current is a *surface*-function, more accurately, a function in which the *environment* of a nerve fiber plays exactly as important a rôle as does the fiber itself. This follows from the fact that the "nerve impulse" is an electric current which enters the fiber from the environment in one region and returns to the environment in another region nearby, the circuit being completed outside the fiber. Since the current penetrates into the fiber from all sides, and also returns in all directions to the surrounding medium, the fiber is, on the temporary level of the impulse, completely enveloped by the outer part of the current. Inside and outside it flows for a stretch roughly parallel to the fiber,

though, naturally, in opposite directions. Propagation of the impulse means that the current gradually changes its location along the fiber, and correspondingly, of course, in the surrounding medium. We need not discuss the causes which produce the current—a local alteration of the surface of the fiber seems to be the essential condition; nor shall we explain why the current migrates from one part of the fiber to the next. What interests us mainly is the fact that, according to a simple rule of physics, there is invariably just as much of the impulse or current outside the nerve fiber as passes through the fiber itself. Now, distances between the fibers of one nerve, the optic nerve for example, are small. If, therefore, at a given level and at a given time, impulses travel along several parallel fibers, the current in *one* direction will be distributed over the various separate fibers through which it passes, while in the opposite direction the flow will fill the common continuous medium in which the fibers are embedded. Outside the group of fibers the impulses themselves form in this manner a functional continuum. To this extent it is obviously incorrect to say that nerve activity is always a function of separate conducting elements.

We are, however, in the habit of concentrating our attention on the distinct histological entities, i.e., the fibers of a nerve. Thus we shall be inclined to say that after all the surface of the *fiber* has to be altered, if a local current, and thereby the impulse, is to be started. Yet from a purely physical point of view it may be doubted whether, so far as function is concerned, the surface in question is more a boundary of the fiber than it is a boundary of the medium around the fiber. To say that, in a nerve, fibers are embedded in a medium is doubtless correct. Yet it is no less correct to say that in a nerve a

common continuous medium is perforated by fibers. As psychologists would say, the difference of formulation means that in one description the fibers are regarded as "figures" and the medium between them as mere "ground," while in the other description this relation is inverted. Whether or not the distinction between these two descriptions has any objective significance remains to be seen. In the meantime it would be an unwarranted preference, if we were, as is still our tendency, to regard the current inside the fiber as important and more or less to ignore the current outside. There is in any case no justification for the belief that nerve function is a matter of separate histological elements, and that thus this function itself is split up into correspondingly separate events.

The function of ganglionic layers is at present regarded as similar to that of fibers in one essential respect: it, too, seems to be first and foremost "surface"-function. With one difference, however. While no evidence points as yet to the assumption that an impulse which travels along one fiber can actually stimulate other fibers which extend parallel to its own direction, Adrian and his collaborators have found that in ganglionic tissue groups of cells will under certain conditions show clearly interrelated activity. This is true of the retina, and also of cortical layers.[9] For such areas, therefore, the notion can no longer be held that each individual cell functions independently. It seems plausible that interdependence of function in active *groups* of cells is brought about by the fact that function of the cells is function of their common environment at the same time. This assumption is corroborated by more recent investigations of the manner in which a nerve impulse is transmitted at a synapse from one neuron to

[9] E. D. Adrian and R. Matthews. *Journ. Physiol. 65* (1928), pp. 273 ff.

another. Experimental data point to the explanation that when the impulse reaches the end of the first neuron a chemical substance is secreted by the fiber. The second neuron is stimulated when this substance has passed through the minute stretch of common medium between the first and the second.[10] In ganglionic layers, therefore, where one set of neurons ends and a new set of cells begins, there is, according to this new discovery, a thin sheet of chemical activity in the common medium. If several neurons of the first set are activated side by side simultaneously, this sheet will extend through the ganglionic area as a continuum; and if impulses follow each other in rapid succession, their individual chemical effects will overlap in time, so that, by summation, a steady chemical state is attained. The particular nature of this state depends on the number of impulses which through one neuron arrive in a unit of time, and on the number of active neurons the mingled end-branches of which feed a common synaptic region.

There is, indeed, nothing in this situation which would make it a plausible assumption that cortical processes consist of independent events in individual cells. In the following paragraphs psychophysical correlates will, therefore, be considered from a *macroscopic* point of view.

When light stimulates the retina, impulses travel first along the optic nerve and then along more central neurons. As they arrive in the visual cortex some property of the impulses in question doubtless represents the kind of retinal process (and therefore of light) by which the impulses have been caused. When we realize how many

[10] G. H. Parker, *The Origin, Plan and Operational Modes of the Nervous System* 1934. W. B. Cannon and A. Rosenblueth, *Autonomic Neuro-Effector Systems*. 1937.

different qualities of color occur in normal vision we are led to believe that among all known classes of physical events only chemical reactions occur in a corresponding variety of nuances. Since the correlates of color must be variable in just as many dimensions as are the colors themselves, we shall therefore assume that chemical reactions are the psychophysical processes which underlie the various qualities of color. These reactions, we suppose, are produced when impulses of optic fibers reach the striate area.[11]

Take the very simple case in which the visual quality in question is a grey which fills the visual field uniformly. The cortical correlate of this grey will be a particular chemical reaction which is in all parts of the visual cortex maintained as a continuous state.[12]—A more interesting situation arises if peripheral stimulation is inhomogeneous. Suppose that a simple white figure, a circle or a square, is projected upon one part of the retina, while around this figure stimulation corresponds to grey as before. When nerve impulses arrive in the cortex both from the figure and from its environment two different chemical processes will be produced, one as the correlate of white and the other as that of grey. The first will occupy a circumscribed region, and the second will extend around the first through the rest of the cortical field. Between the two there will be functional contact along their common boundary. The tissue is an electrolyte; among the components which take part in the chemical reactions there will be ions. Moreover, a given chemical process will be characterized by a particular concentration of participating ions which re-

[11] *Cf* p. 212, this chapter.

[12] The visual cortex contains several layers. We need not decide in which of these layers the reactions may actually occur.

mains constant so long as the reaction does not change. This gives us the following situation: Outside the common boundary of figure and environment there is one concentration of reacting ions, and inside it a different second concentration.[13] Any such difference in the ionic constitution of the two regions leads at once to electric events. The area of the figure assumes as a whole one electrostatic potential, and the environment another. Along their common boundary the potential jumps from one value to the other, and the amount of this abrupt gradient is determined by the difference of ionic concentration inside and outside the contour. It would lead us too far into technical discussions if I were to try to explain why differences of chemical constitution are, as a rule, associated with such electric facts. Nernst has given the explanation in terms of ionic diffusion at the common boundary. His theory has been generally accepted in physical chemistry.

A difference of potential is an electromotive force. Thus, if certain further conditions are fulfilled, a current will flow through both the figure and its environment.[14] Its distribution, the course which it takes, is of considerable interest. It will penetrate the circumscribed area of the figure, it will then spread widely through the homogeneous environment, and will eventually return to the figure. In this manner the figure will be surrounded by a "halo" of current which by its characteristic distribution represents the figure outside its own limited area. I am inclined

[13] Instead of a difference in concentration it might also—but not very plausibly—be assumed that on both sides of the boundary ions are qualitatively different. The conclusion which we have to draw does not depend upon this alternative.

[14] The electromotive force at the boundary of two solutions is unable to produce a current unless the two solutions are in contact with a third medium so that further electromotive forces are added to the first. In cortical tissue this condition will practically always be fulfilled.

to believe that in any further development of psychophysical theory such currents will play an essential rôle. At present many physiologists and psychologists are busy studying *waves* of electric activity which the human brain yields continually. We are not sure to what extent such waves are phases of psychophysical processes, for instance, in the striate area. There is a suspicion that at least some of these waves accompany vegetative processes rather than mental events. Perhaps more attention ought to be given to steady currents. For psychophysical function proper such currents might be just as important as are those interesting waves.

IV

Practically any part of human experience might be taken as an example of the fact that molecular events in the brain do not as such show much resemblance with phenomena. The visual field, for instance, is a continuum. Displacements of particles, on the other hand, may from a merely microscopic point of view appear as a mass of incoherent events. The visual field, however, is seldom a *uniform* continuum. Although coherent throughout, it exhibits individual units such as patches, figures, thing-percepts, which are to some degree detached from each other or from a general background. For this aspect of vision again no analogue is found in a strictly microscopic exploration of cortical events. Such observation would regard as unitary data the movements of single particles or perhaps the interaction of a few; but it could not find any larger entities which, within a widespread field, would constitute specific units of state or process. The situation is not changed if instead of molecular events "the processes

inside single cells" are considered as the correlates of visual experience. So long as we suppose that such processes are independent single events, any pattern in which they may occur is a mere mosaic. Nothing in this mosaic corresponds with the continuity of the visual field, or with the segregation of such specific entities as figures and things.

Macroscopic physical states, however, *are* continua. Any macroscopic physical object, we have seen, coheres because its particles are "in the field" of their neighbors. Chemical reactions which occur in a macroscopic volume are coherent throughout for the same reason. If, therefore, the correlate of a uniform grey field of vision is a uniform chemical reaction extending through the visual cortex, cohesion or continuity is a characteristic both of the perceptual field and of its psychophysical counterpart. But now let a white figure be shown, which visually appears as a segregated part of the field. At once the cortical continuum is thoroughly altered. The area which corresponds to the figure becomes one unit of electrostatic behavior and the environment another. It is not as though functional cohesion throughout the visual cortex were destroyed. On the contrary, it is precisely functional interrelation which leads immediately to electric displacements inside and outside the figure, and thus to the segregation of two areas which maintain each other at different potentials. Using the terminology of earlier paragraphs we may therefore say that segregation occurs as a case of self-distribution of electrostatic potential.[15] This segregation

[15] Those who regard "potential" as a mere construct of mathematical physics with no genuine counterpart in physical reality, may describe the same facts in terms of surface-charges and their fields which appear when in one region the chemical state differs from that in another region. No relevant aspect of the situation would be changed by this procedure. In terms of electric potential the description is, however, much simpler.

of one area from the rest of the cortical field corresponds with the fact that phenomenally the figure is seen as one thing by itself.

If, instead of a white figure, a solid object of the same color interrupts the uniform field, the three-dimensional appearance of the thing-percept offers a new problem in psychophysics. Apart from this the situation is not essentially altered: Again there is in a circumscribed region of the cortex one chemical reaction, and around it there is another. Segregation of one macroscopic unit from its environment will be the consequence. Whatever the correlate of visual depth may be, the psychophysical context shows once more a pattern which in a basic point agrees with that of associated experience.

No patch, figure or thing seems ever to appear as a visual unit by itself unless there is at least *some* boundary at which the visual quality changes. An abrupt slope of potential in the cortex, a functional boundary, is thus a characteristic which accompanies all segregation of visual objects. Although we fully realize how many complicating circumstances arise in individual cases, we may therefore formulate it as a rule that segregation of particular macroscopic entities within a larger continuum is a common trait of visual experience and of its cortical counterpart.

Continuity is a *structural* trait of the visual field. It is also a *structural* fact that in this field circumscribed particular percepts are segregated as patches, figures and things. In both characteristics, we have found, the macroscopic aspect of cortical processes resembles visual experience. To this extent, therefore, vision and its cortical correlate are *isomorphic*. In the last chapter the same term has been used. There, however, it applied to the relation between visual organization on the one hand and the mac-

roscopic structure of situations in physical space on the other. The fact which mediates between the physical and the perceptual structure is now found to be *cortical* organization, which, as a rule, resembles both. Eddington's elephant, for example, is a macroscopic object, a separate entity in physical space. If an image of the animal is projected upon my retina, cortical processes within a circumscribed region of my brain are immediately segregated as a particular macroscopic unit, which is my "psychophysical elephant"; and one phenomenal thing, the elephant-percept, appears in my visual field. Three people walk before me on the physical street as distinct physical entities; correspondingly there are three psychophysical units in my cortex and three people-percepts in my visual space. Physically my own organism is a macroscopic object; I also see it as something apart, namely the visual self; and in my visual cortex its correlate occupies a region in which, as a macroscopic state, it is set apart from the general psychophysical context.—Where perceptual organization does not agree with facts in physical space, cortical organization seems to agree with perception rather than with physics. When two objects, seen at night and at a distance, occasionally melt into one strange visual bulk, stimulation has as a rule not been different enough at their common boundary, and in the visual cortex one large unit has been established which is the correlate of my strange percept.

Our present discussion is mainly concerned with the question of isomorphism between the visual field and its psychophysical correlate. It is not necessary, however, to investigate at present just how far such isomorphism actually extends. Once such structural resemblance is proved to be possible to any extent whatsoever, we can no longer

maintain that knowledge of cortical events will never reveal data of psychological interest. Supposing that observation were adequately directed toward the macroscopic characteristics of psychophysical processes in a visual cortex, the description of these processes in macroscopic terms would be a correct report, not only of the cortical situation, but also of some essential traits of the visual field to which the psychophysical situation corresponds.

Not for a moment should we forget, however, that isomorphism, thus considered, is a relation between visual experience and *dynamic* realities. The distribution of processes in the visual cortex has once before been discussed in these chapters.[16] But the description that was then given referred to the merely *geometrical* aspect which the pattern of cortical events exhibits. I wanted to show that localization of thing-percepts outside the body-percept is altogether compatible with neurological knowledge. Just as in visual space thing-percepts are localized outside the body-percept, so processes corresponding to things are, in the visual cortex, localized outside the processes which correspond to the visible self. So far as it went, this description was correct. And yet it was not quite adequate. From a merely anatomical and geometrical point of view we were not really entitled to speak of specific processes which correspond with things or with the visible self. Connection between the retina and the cortex, as it was then presented, preserves a merely geometrical order of *points;* but it does not provide for any organization. There is nothing in this scheme which could explain, why things or why the body are segregated entities in the visual field. Although it gives an orderly mosaic of local brain-events, it does not lead us to expect either a continuum of coherent function, which

[16] *Cf* ch 4, pp 131-140.

extends through the visual cortex as a whole, or the fact that, within certain areas, there are specific macroscopic units, the segregated processes which correspond to things and to the self. In this respect the situation has been greatly changed by the discussion of the last paragraphs.

In the description to which I have just referred there was, however, one more term that needs a certain correction. Thing-processes, we said, occur *outside* the body-process. What does "outside" mean in this sentence? As a matter of fact, the correlate of a thing-percept and that of the body-percept will quite generally have different locations in the visual cortex. But, if we wish to think consistently, we cannot contend that this geometrical fact has as such any psychophysical significance. Rather such terms as "being outside" have to be given a *functional* interpretation, just as has been done with the terms "continuous" and "segregated." In assigning isomorphic correlates to the various structural properties of visual space we cannot in one case point to functional realities and in another case to mere geometrical relations.

Geometrically one thing will be said to be "inside" another thing, if any line which I draw beyond the boundary of the first enters or passes the second. In a continuum of function a particular region is *functionally* "inside" another, if any functional influence which is exerted by the first alters the second before it has any more remote effects. Conversely, one area is *functionally* altogether "outside" a second area, if interaction between the first and the second is mediated by alterations in a third area. Here I have omitted the special case of direct neighborhood. Obviously two areas are functionally "immediate neighbors" of each other, if an influence which one exerts upon the other need not pass through and alter any third

region; in other words, if those areas are somewhere in direct functional contact.

In most or all cases, it is true, in which these functional definitions are applicable the corresponding geometrical relationships will also obtain. Such is the connection between continuity of physical function and the properties of physical space. And yet the two kinds of definitions point toward two actually different sets of relations. All

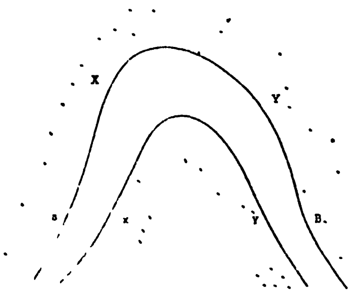

FIG 1

functional relations could, for instance, completely disappear from a medium, while corresponding geometrical relations would remain entirely unaltered. Besides we need only take one more step in order to see that discrepancies between the geometrical and the functional definition of a relation are by no means impossible. The functional meaning of the term "between" may serve as an example. A point X lies functionally between two other points A and B, if the most direct functional influence which A can exert on B (or B on A) will alter X first and only then B (or A). Suppose now that in some region of the brain one part of the cortex protrudes into the recess formed by another part of the cortex (*Cf.* fig. 1). If A and B are two points of the cortex in which we are interested, what points

are functionally or psychophysically "between" A and B? Geometrically the protruding part of the cortex and more particularly the points x and y are in this position. But an influence which A has on B may never pass through x and y, which lie perhaps at an enormous functional distance both from A and B. On the other hand X and Y are likely to be altered by a change in A, before, precisely by this alteration, the change of A has any influence on B. Thus X and Y rather than x and y are "between" A and B in the only sense in which this term should be applied to psychophysical processes.

In this manner, not in a geometrical interpretation, are relationships within the cortical continuum assumed to be isomorphic with the structural properties of visual space. More especially thing-percepts are seen completely outside the body-percept whenever other visual processes intervene between the correlates of those things and the correlate of the visual self. A thing-percept touches the visual self, when at least at one point functional contact is direct between the cortical counterpart of the thing and that of the self. The discussion of relative cortical localization which I have given in the fourth chapter seems to me acceptable only if it is thus understood.

Being "inside" or "outside," "between," "in contact with," "at a distance from" are relationships which the mathematicians call *topological*. From other spatial relations they are distinguished by the fact that in a given case they may remain invariant, while the *metrical* properties of the medium in question are radically changed. Visual space has certain metrical traits, and metrical relations obtain to some extent between its parts. It is not necessary for our main purpose that the psychophysical counterparts of these relations be here considered. It needs hardly to be

mentioned, however, that a more complete theory of psychophysical space would have to contain definite prescriptions according to which distances and other quantitative phases of extension are to be measured in terms of cortical *function*. I doubt very much whether psychophysical distances in a functional sense of the word are proportional to geometrical distances in brain-tissue; nor should I assume that the functional distance between two given points in cortical tissue has once for all a constant value. A great difficulty for any psychophysical theory of visual space is sometimes found in the fact that the striate area as a whole consists of two parts, one of which belongs to the right and the other to the left hemisphere. Since between these parts there is a functional connection through the *corpus callosum*, the existence of a theoretical difficulty can neither be maintained nor denied until the metrical properties of psychophysical space are functionally defined.

V

I have sometimes heard it said that the theory of psychophysical isomorphism is a verbal rather than a substantial achievement. It is easy, thus some seem to feel, to abstract from psychological facts their structural characteristics, and then to describe such psychological structures twice: first in psychological terms, then once more in a language which has a more physiological sound. It is obviously the import of this criticism that the theory does not really contain statements about both physical facts and psychological data; that actually there is only one single set of factual statements which by the use of two different sets of expressions assumes a dual appearance.

In answering this objection we should be clear about its meaning. It is not said that the theory makes unconvincing assumptions about the psychophysical correlates of phenomenal data; rather, it is maintained that actually there are no such assumptions at all.—I cannot accept this criticism. It seems to ignore the fact that the main terms of physiological and physical science have meanings which were established long before there was any theory of psychophysical isomorphism. For a long time it was regarded as an obvious fact that no statement formulated in such terms could ever be analogous to the statements in which psychologists describe the structure of phenomena. The theory of psychophysical isomorphism produces statements such as used to be considered impossible. And it is not a trick of language by which this is achieved. To be sure, expressions do appear in the theory which have not hitherto been used in discussions of cortical events. Instead of being only a matter of language, however, this emergence of new terms indicates that certain phases of physical nature which have been overlooked in earlier discussions are now believed to participate in psychophysical function. What in fact could be meant by those statements in which philosophical Monists maintained that cortical processes and psychological facts might, somehow, be "the same thing"? Practically nothing, so long as *in concreto* the data of these two realms appeared as utterly different. Even the thesis of isomorphism, which is a much more definite proposition, is not yet a theory, but remains a postulate, until we are enabled to point toward modes of physical function that have a genuine *structure*. It is not merely a new expression: the contention that macroscopic physical states rather than microscopic events are the correlates of phenomenal contexts is a positive hypothesis.

Such macroscopic states do have specific structures. And if we try to show that such states may plausibly be assumed to occur in cortical tissue, we are again not dealing with mere expressions; we are discussing physiological facts which may or may not agree with our physical assumption.

It will give us a better view of the situation if one more remark is added. Isomorphism is a postulate. It becomes a theory by virtue not of one hypothesis, but of a whole set of definite assumptions. It is by one such assumption that a cortical correlate of visual continuity is indicated; a second hypothesis refers to the correlate of visual segregation, a third to the isomorphic representation of topological relations in visual space; a fourth will be needed in the case of metrical relations, a fifth for the third dimension of space, and so on for all discernible structural characteristics of the phenomenal world.

On the other hand, our freedom in introducing such hypotheses is greatly restricted. Not only must they agree with available physical and physiological evidence; they must also be mutually compatible. The principle by which isomorphism as to segregation is explained has to be in harmony with that by which cortical function is made a continuum. In the case of metrical spatial relations no hypothesis will be acceptable which is at odds with the explanation of topological isomorphism. Again, whoever tries to offer an isomorphic correlate of visual depth may find that only few possibilities are left for this endeavor; because the previous assumptions set so many conditions with which this further move must be consistent. It is also imaginable that at this point of the procedure not even one possibility of any further move will be left. In this case the theory of isomorphism would have reached the limits of its applicability. I do not believe that this

will really happen. But that it could happen shows once more clearly how little a matter of mere expressions the theory actually is.—

One more step in isomorphic theory will lead us back to our main issue. According to phenomenological analysis requiredness occurs within contexts as a special form of reference or relation between their parts. As yet we have been dealing with only such relationships as are independent of any concrete properties which their objects or terms may possess. In the next paragraphs other relations will be discussed which obtain between the terms inasmuch as these objects have definite characteristics. Requiredness belongs to this class as its probably most interesting special case. If, therefore, it should be unthinkable in principle that any such relations have, as experiences, isomorphic correlates in cortical function, the same would be true of requiredness. We have thus good reasons for investigating this further problem in the theory of isomorphism. Natural caution makes it advisable to exclude at first the case of requiredness itself and to investigate more elementary relations of this class.

A relation, I once read, may be called a "product" of the items or terms of which it is predicated. This particular product, however, has properties by which it differs greatly from other things to which we give the same name. Hydrochloric acid is the product of chlorine and hydrogen. When the product is formed these substances disappear, and we get instead another chemical substance, i.e., an entity of the same general class. Or take a slightly different example: two animals of opposite sex mate. The offspring may be called their product; it would be more correct, however, to say that the young are products of male and female germ-cells. In this case again the producing components

disappear in the product, and the product belongs to the same general class as do the components. If this be taken as typical of production in the physical world, the production of relations in experience has little in common with physical production: The terms do not disappear when they "produce" the relation; and the relation, their product, is not an entity of the same class. In a way, it is true, the terms "unite" when their relation is experienced. Still they remain two distinct data. The relation itself, on the other hand, may be a "product" of the terms; but it belongs to another class, as shown most clearly by the fact that it has no proper life of its own. If we separate the terms, we do not cut their relation; we simply make it vanish altogether. Has ever anything like this been found in nature?

So far the argument. In psychology aspects of experience which have such characteristics are well known as Ehrenfels-qualities. Von Ehrenfels himself pointed out that relations satisfy the criteria by which his "Gestaltqualitäten" can be recognized. I hesitate to concede that no situation in nature exhibits such characteristics.

But first let us consider an example which I take from the field of perception. When looking at two parallel lines of slightly different length we find that the appearance of the pair tells us directly: the line to the right is longer (*Cf.* fig. 2). *Where* do we see that? In the present case we see it at the upper and the lower ends of the lines. When we compare these lines, they are not actually seen as two altogether separate things, but rather as the left and the right edges of the area between them. Where the lines end the area also ends, although it has no distinct edges here. The area converges to the left and diverges to the right. In other words, there are slopes from the ends of one line

to the corresponding ends of the other. This, however, is equivalent to saying that the line to the right is longer. The first two expressions seem to come nearer the actual phenomenological foundation of our judgment; the last formulation, which corresponds better with common parlance, refers more directly to those objects in which we are interested, namely, in the present case, the lines. All three statements point to the same essential fact, although they describe it from slightly different points of view.

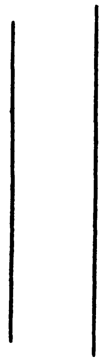

Fig 2

The slopes between the ends of the lines are Ehrenfels-qualities of the pair which originates as a perceptual unit when the lines are brought together. Divergence or convergence does not belong to the same class of perceptual facts as do lines or lengths of lines. Moreover, in the unit which they form the lines do of course not disappear. Lastly, the slopes between their ends have no independent existence; if the lines are separated no slopes are left, and no divergence or convergence. Thus our description of a very simple instance contains all the characteristics which have been regarded as peculiar to perceived relations.

A slope, however, is a particular case of a "gradient,"

and gradients do not occur only as aspects of perceptual or other phenomenal situations. Physical situations exhibit gradients so regularly that the term as such is much more often used in physics than it is in psychology. Improving on an earlier theory which I had proposed some time ago Dr. von Lauenstein has therefore pointed out that gradients in psychophysical contexts might be the correlates of such perceptual relations as the one which we have just discussed.[17]

What is meant by a gradient in physics? Examples will tell us more than formal definitions. Between two objects of different electrostatic potential a "field"-gradient and, under certain conditions, a corresponding current develop. Between two objects of different temperature a temperature-gradient and a heat-current are established. In both cases the differences between two physical states are, one might say, realized or functionalized by specific events which come into existence between them. In von Lauenstein's theory it is plausibly assumed that when two phenomenal data differ in some respect their cortical correlates are also different physical states. In a medium like brain tissue, however, two simultaneous psychophysical processes are not functionally separate events. Thus a gradient may be established between them which owes its origin to the difference between the two processes. According to the nature of these processes and of their difference there will be gradients of different kinds, which in this theory are held to be the correlates of all those various relations found in perceptual fields.[18]

Comparison between perceptual relations and physical

[17] O. Lauenstein, *Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler*, Psychol. Forsch. *17* (1932), pp. 130 ff.

[18] In the next chapter it will be explained how by the same hypothesis successive relations are interpreted.

gradients reveals that both have the same structural nature. A gradient may be said to be a "product" of the different physical states between which it develops. In this product the states themselves do not disappear. In a way they are united by the gradient, they become members of a larger functional context; but they remain two distinct data. The gradient on the other hand does not belong to the same class of entities as the states between which it originates; a current for instance is not an object nor a potential nor a temperature. Again, gradients have no existence of their own. If the states or objects are separated between which a gradient has been established, it vanishes altogether. It seems safe to conclude that there are situations in nature which exhibit in the form of gradients the same structural aspects as do relations in perception. Gradients are therefore isomorphic with such relations. If an observer of macroscopic brain-events should mention in his report that between two different states he found a gradient depending upon their difference, he would thereby mention a structural trait of the situation which a relation in the phenomenal field and the correlate of this relation have in common.

In applying this notion to psychophysical problems we need not at once specify just what gradients may be the correlates of various relations. Without any such assumptions we are in a position to derive from our hypothesis one consequence which may at once be tested. Whatever other properties a gradient may have, the term has its accepted meaning only so long as by a gradient we understand a physical observable that "falls off" through space from one thing to another. If this be true, the slope of any gradient between two given objects or states must depend not only upon the actual difference between their proper-

ties, but also upon their distance from each other. An increase of this distance, for instance, would make the gradient less steep. Take the example of the two parallel lines. If their length is slightly different the area which they include is convergent or divergent, i.e., it is a trapezoid, only when their distance is short. With increasing distance the slopes which lead from the ends of one line to the ends of the other, will gradually become less conspicuous, and eventually the observer will be unable to perceive them. Phenomenally the area will then be a rectangle, an oblong. Consequently, an objective difference of the lines which was just noticeable at a shorter distance will become imperceptible at a certain larger distance. Only by increasing the objective difference between the two lengths could noticeable slopes be reëstablished under these conditions. The just noticeable difference is therefore, in this case, a function of the distance between the lines.

If, however, von Lauenstein's hypothesis is correct the same conclusion should apply quite generally. Being the phenomenal counterparts of gradients, all such perceptual relations should in the range of liminal differences depend on the distances between their terms. In this manner the assumption could be tested experimentally. As yet data are available for only one particular relation, namely, that of comparative brightness. Under the guidance of Dr. Metzger, Miss Jacobs measured the just noticeable difference of brightness between two surfaces which were exposed at varying distances from each other.[19]

The experimental procedure was this: The subject fix-

[19] M. Jacobs, *Ueber den Einfluss des phanomenalen Abstandes auf die Unterschiedsschwelle fur Helligkeiten.* Psychol. Forsch. *18*, (1933), pp. 109 ff

ated a given point, and two objects of slightly different brightness were shown simultaneously, one on the left, the other symmetrically on the right side of the fixation mark. The just noticeable difference was determined for three distances between the objects. About technical details Miss Jacobs' paper should be consulted. As a main precaution such positions of the objects were chosen that their images were projected upon retinal points of equal sensitivity.

For a group of ten observers the average noticeable differences were, for the distances of

10	80	200 cms
11.1	23.5	45.2,

and, in a second experiment with a slightly different procedure,

10.9	23.5	45.3.[20]

This result agrees completely with the conclusion which we have drawn from the hypothesis, that a gradient is the psychophysical correlate of brightness-difference as a perceptual datum. With increasing distance between the terms the just noticeable difference grows considerably.—The experiments have been repeated several times and by several students. Under the simple conditions just referred to their outcome has always been the same.

In applying the gradient theory to the present case it has been assumed that, if other circumstances are constant, the distance between two cortical processes will be the greater, the farther apart the retinal images are with which those processes correspond. Under the experimental conditions which yielded the data of these experiments, this seems to be a safe assumption.

20 All thresholds were measured in the same arbitrary unit. The statistical reliability of the differences found by Miss Jacobs is discussed in her paper.

CHAPTER VII

On Memory and on Transcendence

I

Common language uses the term memory in a specific and narrow meaning. To speak of memory seems natural only when we are occupied with the past. "I am sure I posted your letter," "I still remember Professor Stumpf's seminars"—in such statements we refer to facts of memory. It does not seem right to apply the same word to day-dreaming, to other facts of imagination and to dreaming during sleep. Parts of our past, it is true, may as such be revived in these situations, but they are revived more or less incidentally, and there is no intention to copy what has actually happened in previous life. Nevertheless it is obvious that most of the "material" which appears in such processes could never be dreamt or imagined, unless experiences of a similar kind in principle had occurred in our past. Although the content of dreams and imaginations may not be localized in the past, it is at least obviously dependent on events which often lie far back in time. In psychology we have no adequate term for this influence of the past which is akin to memory, although it is not memory in the usual sense. The whole field of this science, however, is pervaded by facts of which the same may be said. When operating with concepts, as we do in science, or when thinking about the present economic situation in this country, we are not directed

toward the past; and yet practically all the constructs and notions with which we are occupied during such activities are at our disposal only as gifts of the past. The meanings of words which we read have no particular temporal location, unless it be that given by their present context; but if in the past we had not learnt the language in question the same words would be unintelligible. In the same manner we speak and we write fluently, for the most part without any reference to the origin of these achievements which, as such, is a matter of the past. In this particular connection many psychologists use the term "habits"; but this word, too, has too narrow a connotation for our purpose. Who would call it a habit that I can operate with such concepts as "relation" or "entropy"? Besides, not only are countless activities automatic now, because they have been shaped in earlier times; but the same holds for what one might call a secondary organization of *perceptual* fields. I do not usually think of the past when I walk through New York City, say, from Grand Central Station to some other well-known building; but it is nevertheless experiences of the past which make me turn here to the left, there to the right, because just these streets stand out as "the right way."

That it is awkward to speak of memory in all such cases has been observed by several authors. On the other hand, it cannot be denied that there is one point which these facts have in common with memory in the usual sense: the participation of previously established conditions in the determination of present events. With this point the following discussion will be mainly concerned.

What has happened in our past life could hardly codetermine our present activities unless to some extent the past were preserved far beyond the time of its occurrence.

We clearly mean this when we say that events of the past become conditions of present mental processes. Much time may pass between an original experience and the moment in which there is unmistakable evidence of its delayed effect. Some authors seem to think that we need not assume any entity which survives during the interval as a representative of that previous experience, and which becomes effective when present circumstances are favorable. They ought to realize what this view implies: a first event would influence a second, even though between the two there is an empty period, no connection and no continuity, sometimes for hours and days, occasionally for years. I should hesitate to adopt this notion which is so strikingly at odds with all our fundamental ideas of functional interdependence or causation. Other psychologists do admit that without some remnant of its occurrence a past experience could not influence what happens now. They add, however, that the remnant which satisfies their need for continuity is a *mental* entity, that it belongs to the same general class as actual experience. I must confess —and here I differ widely from many philosophers and psychologists—that I find it difficult to grasp the meaning of this assumption. I know the world of experience, of phenomena; I admit the existence of a second world, the world of nature. But I fail to see what entities there may be which are said to belong to the former of these worlds, while at the same time they are never there. The remnants of past experience never do occur as such in actual experience; they do not really belong to the phenomenal world, so much is certain. Under these circumstances, what positive meaning is implied in the statement that they are mental entities? It is expressly denied that they are parts of the physical world. Do we know of a third world, the

mental, the properties of which we find by some procedure of construction comparable to that of the physicist when he infers the properties of nature? As a matter of fact we do infer, and at the present time we *only* infer something when we speak of remnants of past experience. But as a scientist I refrain from burdening the system of my thought with the tremendous hypothesis that here my inferences refer to a third realm of existence, different from nature and yet not phenomenal. If there are any remnants which mediate between my previous mental life and a present recall (or any "memory" in the wider sense), such remnants belong to a world outside the phenomenal realm. Outside this realm I have already assumed nature, more particularly a nervous system in which the correlates of previous experience have occurred. It seems therefore proper to follow the rule that worlds ought not to be multiplied beyond strict necessity, and to construct the remnants of past experience as entities of the physical world, namely of the nervous system. So long as this attempt has not yet been shown to be futile it would not be a sound procedure to assume that these remnants are parts of a third, a never experienced mental world.

The obvious way of connecting the present problem with our previous assumptions is this: Neural events tend to modify slightly the state of the tissue in which they occur. Such changes will resemble those processes by which they have been produced both in their pattern and with respect to other properties. They are to that extent representatives of the past. As such they will be able to codetermine actual processes of the future. It is true that metabolism and other subsequent influences are apt to obliterate most of these remnants. Even those which survive may as a rule be gradually altered. But this is pre-

cisely what should happen if our assumption is to fit the facts of memory. So far as observation goes most of our experiences never have any effects in later life; nor does it matter, since so many have no importance whatsoever. And to recall previous events exactly as they were originally is probably not possible in a single case.

This, like psychophysical isomorphism, is an hypothesis. It has been criticized on the ground that no such traces of neural events can yet be demonstrated by neurological methods, and it has almost become a fashion to deride it. But even if no direct neurological evidence were available by which the hypothesis could be supported, would that constitute an argument against its scientific relevance? May I make a more general remark. Nations, we hear, will never learn from their own history: a bad example for scientists to follow. Here is one precedent which might serve as a lesson: If we neglect older and more metaphysical speculations, atomic physics was a matter of "construction" from the middle of the 18th till the end of the 19th century. Up to this time the atomic constitution of the physical world could not be directly demonstrated. Atomism was an hypothesis the validity of which rested entirely on indirect though often striking verification. No such verification satisfied those Purists among the physicists and philosophers who, even toward the end of the 19th century, were still regarding atoms as mere fictions of their more naive colleagues, because no atomic events were among the observables of the time. Then, in the investigation of electric conduction in rarefied gases and of radioactivity, atomistic theory proved so successful that the opponents became less outspoken. Their arguments were quickly forgotten when eventually one direct demonstration of atomic events after another was discovered.

Was it wrong to be an atomist before this time, and did it suddenly become the right attitude in those eventful years? Those who had adopted atomism as their working hypothesis had pushed steadily on accumulating indirect verifications, until by their very work the time had become ripe for final direct decisions and for their immediate understanding. Meanwhile their opponents had had the full satisfaction and comfort of never having touched anything that was slightly dubious. It is most unfortunate that the history of science should afterwards speak of them mainly as of men whose opposition almost postponed important discoveries.

Why should this happen again? In the present period of psychological thinking the nature of hypotheses does not seem to be fully and generally understood. An hypothesis is not a poisonous substance, nor is it a minor crime which some scientists commit in hours of carelessness. Hypotheses belong to the more important tools of research, and few major advances in science have ever been made without the help of such assumptions about possible but not yet observable facts. We are asked to give direct evidence that the hypothesis of neural traces is correct. I have never met any hypothesis for which this can be done. When it becomes possible the hypothesis as such disappears; it is transformed into something else. Until then it energized the work, it furnished the experimental questions of those who *wanted* to verify it, indirectly at first and then wherever possible directly. To demand that an hypothesis be at once proved in direct observation lest it remain irrelevant, is tantamount to saying that all hypotheses without exception are irrelevant. All hypotheses, it is true, may be wrong. But those scientists who never take the risk of making mistakes will hardly ever

make discoveries either. Discoveries are usually made by those who try to test an hypothesis. To conclude: The assumption that neural remnants mediate between past events and present experience cannot reasonably be criticized just because it is an hypothesis. Whoever does so takes implicitly (and anachronistically) sides with those who before 1900 objected to atomistic ideas in physics for the same strange reason. It is perfectly admissible, it is even necessary that those who do research often think far ahead of actual knowledge. I regret that in psychology this is not yet a truism.

As a matter of fact, the question is not whether neural events change the status of the tissue in which they occur. The only question which may still be debated is: whether such changes as do undoubtedly occur have the permanence and those other properties which we must attribute to memory-traces. According to our present knowledge the primary effect which nerve impulses produce in ganglionic layers is chemical activity.[1] Such activity in itself alters, for the time being, the status of the layers in which it occurs, and at least for some moments this effect will survive its causes. If there be any precipitation of chemical products, any adsorption of such products on microhistological surfaces, the reëstablishment of previous conditions will be postponed, and complete return to the original status may never be effected in many cases.

From another point of view we are led to similar conclusions. Considered as a conducting medium nervous tissue is an electrolyte, which means that any current which passes through the tissue involves the displacement of ions. It is a peculiarity of electrolytical conduction that surfaces within the medium through which ions cannot

[1] *Cf.* ch. 6, p. 212.

freely pass are, as a rule, at once polarized: Ions are, in minute layers, accumulated and adsorbed on such "interfaces"; in consequence of it new forces arise which counteract those of the current; and owing to the change in ionic concentration chemical reactions may occur at those boundaries.[2] The accumulation of ions at each point of a surface is proportional to the density of current at this point. Thus the current deposits on interfaces in its path an adequate picture of the pattern or distribution with which it passes through these surfaces. As time goes on this curious process of self-registration is continued. If the current remains unaltered, the *same* picture is deposited continually; as soon as the current changes its pattern, a correspondingly *new* design develops on the surfaces. Thus the current writes its own history. Many years ago, when interest in macroscopic physics was more vivid and electrolytical conduction still a comparatively new subject, physicists would find great pleasure in studying the often beautiful records which currents sometimes leave where they pass from an electrolyte into an electrode.

Physiological observation shows that living tissue is strongly polarized by electric currents. Nothing else can be expected in a medium in which homogeneity is an exception and interruption by interfaces the rule. We know, however, that in the nervous system physiological function itself is for the most part associated with currents. Since these do not differ from ordinary physical currents, they have necessarily the same effects. It follows that a current which develops in the nervous system polarizes the inter-

[2] For brevity's sake I apply the term "polarization" to the *complex* of effects which are apt to occur at interfaces In the physicist's terminology polarization is, in this connection, mainly the development of electromotive forces which tend to block the path of the polarizing current.

faces through which it passes, and that the pattern of such polarization is the pattern of the current itself as it passes through the region in question. To the extent in which such self-registration is not deleted by subsequent events, cortical currents will therefore leave their diary spread all over the interfaces of the tissue. As a working hypothesis I assume that by such records of function the gap is bridged between the past and the present. In other words, I am supposing that they are permanent in a degree which equals the comparative permanence of memory.

In all probability remnants of neural function are of course no more "substantially" permanent than is any tissue. Just as the tissue is rather a stationary state, the material of which is continually, though very slowly, replaced by new material, so the traces will be stationary patterns rather than permanent objects. In this respect they differ from common electrolytical remnants, which are more strictly objects.

One might find a difficulty in the fact that in the last paragraphs the existence of two different kinds of traces has been advocated: first, that of remnants which are the representatives of primary chemical activity in the cortex and, secondly, that of traces which currents produce by polarization. In answering this objection I should like to point out that as a matter of fact both assumptions seem to me unavoidable; to that degree the formation of traces is not a matter of arbitrary speculation, but a necessary effect of neural activity. It remains, of course, an open question whether both kinds of remnants have the same degree of permanency. The difficulty on the other hand which such a dualism seems at first to entail will soon disappear, if we realize how the origin and the distribution of polarizing currents is related to chemical activity.

We had better return to a simple example which has been previously considered: A simple white figure is seen surrounded by a uniform gray. In the striate area this visual situation is represented by the fact that one chemical activity takes place in a circumscribed region which corresponds with the figure, while around this region another reaction is maintained which represents the gray ground. I have explained how under these conditions a current will originate which penetrates the figure and envelops it like a "halo" as it returns through the environment. If, now, traces are left both of primary chemical activity and—by polarization—of the current, the total pattern of polarization will, within the figure and outside, truthfully represent what has just happened. But where there are remnants of primary chemical activity they will do exactly the same. Thus there is really no difficulty; the "dualism" of traces only pictures the dualism of previous function, and the former becomes necessary inasmuch as actual function has a dual aspect.

No more than one step remains now to be taken. Not only the traces of primary chemical activity, but also, as a rule, the products of polarization will be different within the region of the figure and outside in its environment. It seems therefore a natural assumption that in the realm of traces similar electrostatic effects will subsist as have been attributed to the pattern of actual function. In the case of an homogeneous field a uniform macroscopic trace-continuum will be left by the continuum of function. But where a special circumscribed region has been detached in function, the same region will remain segregated in the trace-continuum. As to the nature of those forces on which continuity and relative segregation are based no differ-

ence need be assumed between the processes and their remnants.[3]

It seems superfluous to describe further instances. We should only have to repeat in terms of traces what was said in terms of actual processes in the last chapter, when slightly more complicated situations were being discussed. The reason is that, so far as cortical correlates of experience are isomorphic with this experience itself, the same isomorphism obtains between those correlates and their traces. With one essential restriction, however: We cannot assume that such isomorphism will be strictly preserved far beyond the time at which the traces have been formed.

As retinal stimulation changes and one visual field is transformed into another the psychophysical processes will also vary correspondingly. At every moment these processes write records of their own pattern on top of what their antecedents have written. This, however, need not generally lead to a confusion of the records. Just as the various parts of one "simultaneous" layer will not simply penetrate each other, so the record of one moment will not necessarily be confused with that of the next. And there is another analogy between parts of a simultaneous layer and traces which follow each other in the dimension of past time. Suppose that in our simple example the white figure disappears after a while so that its place is filled by the uniform gray of the ground. In the realm of traces the figure is represented by a region which remains detached from its spatial environment, the trace of the gray ground. When the white figure disappears its trace ac-

[3] I prefer to postpone the answer to the question whether in the realm of traces electromotive forces between different regions may produce and maintain feeble currents.

quires a new neighbor, inasmuch as now traces of uniform gray are formed everywhere: the first layer which corresponds to the gray will be in direct contact with the last layer which represents the white of the figure. Similarly on its first appearance in an otherwise homogeneous field a figure will be detached from the homogeneous trace-continuum a part of which it now replaces. It follows that in the dimension of past time the figure will be segregated from subsequent and from previous traces precisely as it is detached from its simultaneous environment. More generally speaking, in the dimension of past time there will be organization which in many respects resembles that assumed by us in spatial extension.

We come then to this conclusion: Processes write their history chronologically, so that, as page is laid on page, each page gives the description of a moment, and the sequence of pages, from bottom to top, tells us of past time from far back up to the present. This metaphor is, however, not altogether adequate: There are no separate pages. Rather, in the dimension of past time the record is a continuum just as it is in its representation of space. And instead of pages, which are indifferent to the content, the record exhibits words, sentences and chapters which are set off from each other precisely according to this content. I shall not deny that much must gradually happen in this curious book, as times goes on. The traces are by no means inert objects. Pervaded by forces, and possibly sometimes by currents, the record will tend to revise its own text spontaneously. If we consider that it is also under the permanent dissolving influence of metabolism, we can only expect the final edition to be a sketchy abstract of a distorted story.

It seems advisable to point to some major differences

between older theories of memory-traces and the present hypothesis. Neurology, we remember, was during the last century dominated by machine-conceptions about the function of the nervous system. According to these ideas ganglionic layers consist of many special centers or devices, each with a particular task, which are connected in such a manner that simultaneously and in succession the right coordination is achieved. It was an application of such ideas when memory-traces were said to be preserved in special centers which evolution must have prepared for this particular purpose. Thus a center for visual memory images, different from the visual cortex, was postulated by many. It is obvious that with such a premise a further assumption becomes necessary: evolution must have provided special anatomical and physiological arrangements by which, when something happens in the realm of processes, a memory-trace of the event is deposited in the more or less distant storehouse. The present hypothesis, which does not assume any special devices by which the traces are formed, knows of no such memory centers either. As I see it, processes cannot fail to write their history in a medium which has the general properties of nervous tissue. Strictly speaking, therefore, our hypothesis refers to the relative stability of their script rather than to its existence or occurrence as such, Moreover, such traces of past events are left in just those locations in which corresponding processes have occurred. Thus the remnants of visual experience are in the case of mammals, and particularly of man, localized in the visual cortex, those of auditory perception in the temporal region of the brain, and so forth.[4] The trace of any organized state that extends

[4] It seems quite possible, of course, that the currents, which spread around any segregated unit in a cortical field, penetrate into other areas,

as a process beyond either the visual or the auditory cortex will of course have the same wide extension; it will be spread through such tissue as connects those particular areas with others.

A further difference between the older and the present view is closely related to the first. The theory of special memory centers knew of macroscopic coherent states neither in the case of processes nor in that of their remnants; the theory was, quite naturally, just as atomistic as were the psychological convictions of its authors. No wonder then that the orderly retention of distinct things and events was explained by separate localization of their traces in minor centers. The memory traces of different objects were even said to reside in different single *cells*. At this point the contrast between the present and the older assumption becomes particularly striking. If a process writes an isomorphic record of its own occurrence it does so wherever it happens to take place. True it is that purely visual processes are apparently confined to a particular part of the brain. Within this region, however, the same particular process may at one time spread here and at another time elsewhere. The figure of our paradigm, for instance, will change its cortical location whenever the physical object and, with it, its retinal image moves. Mere movements of the eye, while the object remains stationary, will again lead to corresponding changes in the location of the figure-process. Generally, therefore, a process of given properties, i.e., a visual thing, will not be represented by a single neural remnant only. If it has occurred several times and in different places, remnants will be left of all such oc-

and that thus their polarizing effects or traces have the same wide extension If this be so, it would nevertheless be true that traces are formed precisely where the processes in question occur.

currences. They will have correspondingly different locations in space, of course at different levels in the dimension of past time. The question whether experiences have "localized" memory-traces appears thus as not quite fortunately formulated. The less so, since the trace of a circumscribed entity is likely to be surrounded by a halo of polarization.

Some years ago Professor Wheeler[5] criticized the assumption of neural memory-traces on general and on experimental grounds. At that time the present form of the theory did not yet exist. Meanwhile Professor Koffka[6] has analyzed and answered those objections so carefully that I need not discuss them once more. Neurological evidence, it seems to me, tends to encourage our view. We assume, for instance, that the remnants of processes are left where the processes themselves have occurred. What are the neurological facts? When in man the visual cortex is seriously injured, the visual field loses its organization; it becomes chaotic. But at the same time visual memory is apparently abolished.[7] Again, if rats have acquired a brightness-discrimination habit, this habit disappears when their visual cortex is completely destroyed, although reactions to differences of brightness are as such still possible, so long as the lower visual ganglia remain intact.[8]

[5] R. H. Wheeler and F. T. Perkins, *Principles of Mental Development* (1932), pp. 387 ff.

[6] K. Koffka, *Principles of Gestalt Psychology* (1935), pp. 452 ff. In Koffka's book the reader will find the theory of traces concretely applied to a great many problems with which the present chapter is not concerned. Moreover, in Koffka's discussion of memory the theory itself is much farther developed than has been done by any other author. I refer particularly to his chapters 10-13.

[7] A. Gelb and K. Goldstein, *Psychologische Analysen hirnpathologischer Fälle* (1920), pp. 111 ff.

[8] K. S. Lashley, *The Mechanism of Vision XII* Nervous Structures Concerned in the Acquisition and Retention of Habits Based on Reactions to Light. Compar. Psychol. Monogr. *11* No. 2 (1935), pp. 43 ff.

As early as 1898 von Kries[9] pointed out that recognition of visual objects is possible even if the traces in question have one location and the present processes quite another. This fact can be easily demonstrated even under conditions where no doubt is left as to that difference of localization. Some authors are inclined to think that such observations constitute an argument against the assumption of traces as neural entities which are located where the corresponding processes have occurred. How does it come to pass, they would say, that under these circumstances a process interacts with the "right" trace and not with any others? This argument assumes that interaction between an entity and just one among many others can only occur if the two entities have the same location. It seems to me much more probable that in recognition the selective factor is *resemblance* between the process and the trace rather than identity of location. Here is an analogy: Among many tuning forks only those will resonate with a given sound wave which have approximately the same frequency. And this selective effect will be practically independent of the location of the resonating forks.[10]

The outcome of certain experiments of Lashley's is sometimes said to refute any conceivable theory in which memory traces are regarded as localized neural facts. When rats have learned to take the shortest way through a maze, destruction of minor portions of the cortex does not lead to any appreciable disturbance of the habit. If considerable parts of the cortex are removed, a deterioration of performance is observed. This effect, however, is not related to the *locus* of the destruction; the degree of deterioration appears correlated rather with the amount of tissue that has been destroyed—wherever the injury has taken place.[11] It seems to me that, far from contradicting the theory of neural traces, these results are in harmony with what the theory would make us expect in this case. When learning to run a maze the rat uses many

[9] J. von Kries, *Ueber die materiellen Grundlagen der Bewusstseinserscheinungen.*

[10] W. Kohler and H. von Restorff, *Zur Theorie der Reproduktion.* Psychol. Forsch. *21* (1935), p. 59.

[11] K. S. Lashley, *Brain Mechanisms and Intelligence* (1929). pp. 86 ff.

sensory cues of different modalities. Singly taken, most of these, we know, are not strictly needed in the final performance, although under normal conditions they are helpful. (This is particularly true of vision; the rat can do without it.) Similarly, even during the later phases of learning the animal does not and need not always make precisely the same movements as one trial follows another. The theory of neural traces will therefore conclude that, after the learning is completed, practically all regions of the rat's cortex contain traces; more accurately parts of *contexts* of traces, which refer to the task in question. But for most of these traces there are substitutes in other parts of the cortex, because both the sensory and the motor constituents of the performance have varied so widely during previous trials. For this reason minor local destructions will have no observable effect. On the other hand, the more the destruction extends, the more likely will traces be eliminated for which no fully adequate substitutes are available. Thus for statistical reasons the habit will degenerate in correlation with the *size* of the lesion, whatever its locus may be.

I wish to add a few remarks about the problem of brightness-discrimination.—When the visual cortex of a rat has been completely destroyed, no effect of previous training is left. In Lashley's investigation, however, new training reestablished the discrimination.[12] If these experiments are conclusive, brightness-discrimination is a visual function of which, in the rat, subcortical centers are no less capable than is the striate area. Since excellent vision occurs in birds—who apparently have no visual cortex—this fact as such is hardly disturbing. In mammals, too, visual function passes through lower centers before it reaches the cortex.—It would follow, secondly, that in such centers visual function leaves traces just as it does in the cortex. How else could learning be possible? This involves no difficulty for the hypothesis that traces are neural facts, because from the standpoint of this assumption there is no reason why traces should not be formed in all ganglionic strata whatsoever through which a process passes.—But, thirdly, it appears that after destruction of the striate area new learning

[12] *Cf. The Mechanism of Vision XII.*, pp. 58 ff.

is *needed.* And, if in those lower centers traces *can* be formed, why were they not formed before when the rat still had a visual cortex? It seems that, if they had been formed, no new learning would be necessary. Professor Lashley finds it difficult to explain this fact. A tentative answer may give us a better understanding of the way in which our hypothesis ought to be applied *in concreto.* The rat has to choose between a dark and a light alley. This means that a simple visual situation has to become imbued with a "meaning" and corresponding motor tendencies. The physiological counterpart of this "association" is probably a context which, in the realm of traces, extends from the striate area into other parts of the cortex, first of all into the motor region. Now, in lower visual centers traces may be formed which represent the merely visual aspect of the situation. But so long as actual responses are mediated by the striate area in conjunction with motor centers, those other traces in the lower visual centers will remain purely visual facts to which the training adds no further context. Thus, in spite of the existence of such traces, no "habit" will be left when the striate area is removed. Only after new trials, when lower visual centers begin to operate in conjunction with other and particularly with motor regions, will a trace-context develop which extends from the former to the latter. Then, according to our hypothesis, a discrimination-habit will again emerge.[13]

[13] Lashley believes "that the memory trace is not localized anywhere within the visual cortex" (p 73) He is led to this conclusion by the observation that so long as *any* moderate portion is left of the striate area the brightness-discrimination habit is not seriously disturbed. I am not convinced by this argument. According to our assumption more than one trace of the visual situation is formed in the rat's striate area; there will be as many traces in as many different parts of the visual cortex as during trials there have been different directions of the animal's head and eyes in relation to the stimuli. In other words, the pair of stimuli is represented practically everywhere in the striate area, of course at different levels in the dimension of past time, and even a small group of cells may be large enough to contain an adequate representation of this simple situation: dark *versus* light.—An altogether unexpected discovery by Dr. Krechevsky has in the meantime made us realize that Lashley's work on brightness-discrimination was based on a premise which can perhaps no longer be held I shall therefore abstain from further discussion of his argument

II

May I once more return to problems of perception, and repeat: A visual field contains as a rule circumscribed specific entities which are to some degree detached from other such entities, and from a less differentiated environment. In the formation of these particular units, figures for instance, certain rules seem to be followed. We seldom experience much of the actual genesis of visual percepts, but we can observe what things or figures appear under different conditions of stimulation; and then we can find the rules which connect the first with the second. These I call the rules which the formation of thing-percepts obeys. The first to formulate them with a full realization of their fundamental importance was Wertheimer.[14] He has also given a more general principle of which these rules seem to be more particular expressions. The principle contends that organization of a field tends to be as simple and clear as is compatible with the conditions given in each case.[15] It is not always easy to prove that a particular given organization is actually clearer and simpler than any other organization that would be possible under the same conditions. But sometimes we have a natural standard of reference which gives those terms a definite meaning. A circle is simpler than are other closed figures; a straight line is simpler than are curves or broken contours which extend mainly in one direction; a regular pattern is simpler and

against the existence of localized traces in the visual cortex. *Cf.* I Krechevsky, *Brain Mechanisms and Brightness Discrimination Learning* The Journal of Comp. Psychol. *21* (1936), pp. 405-441.

[14] M. Wertheimer, *Untersuchungen zur Lehre von der Gestalt,* II. Psychol. Forsch. *4* (1923), pp. 301 ff. This paper is probably still the best introduction to the problems of Gestalt Psychology

[15] *Bericht uber den 6. Kongress fur experimentelle Psychologie* (ed. F. Schumann), 1914, pp. 148 ff.

clearer than are others in which such regularity does not altogether prevail. These and similar instances may be profitably used for testing the validity of the principle. If the principle is right, a figure which comes sufficiently near one of those natural standards should, or at least might, appear in perception as though it were the standard itself; organization should, in this sense, be "too good." Confirming observations have been reported from several sides.[16] The following example seems to me particularly instructive.

The faces of other human beings appear to us as a rule bilaterally symmetrical. Occasionally, it is true, we see at once that a nose is strikingly askew or that the outline of one cheek is quite different from that of the other. On the other hand, few of us are aware of the fact that fairly considerable degrees of objective facial asymmetry are the rule rather than an exception. There are two ways of making people realize this asymmetry. The first consists in "psychological analysis"[17] Normally faces are perceived as visual units. For the present purpose, however, we should, for instance, concentrate on the eyes to such a degree that the rest of the face in question becomes almost an indifferent background. Under these circumstances we shall often discover that the two eyes have different sizes, or different shapes, or slightly different heights. A similar result may be found if the same procedure is applied to the ears. Again, when concentrating on the contours of

[16] Professor Gibson's recent discovery (J Gibson, *Journal of Exper. Psychol 16* 1933) does not belong to precisely these facts, because in his observations the standard in question is only gradually approached during prolonged inspection. On the other hand, his experiments corroborate the view that the tendency which we find in immediate perception is also present in the realm of traces. This is just what I shall try to prove in the following pages

[17] *Cf.* W. Kohler, *Gestalt Psychology* (1929), pp. 183 ff.

the two cheeks, we may realize with surprise that they do not actually correspond with each other as they ought to in the case of perfect symmetry. Thus, when perceived quite naturally, faces as wholes have for the most part a symmetrical appearance. And yet a more analytical scrutiny —which interferes of course with natural organization— will often reveal that details of the same faces deviate considerably from such symmetry.[18]

A second method which serves the same purpose uses front-view photographs of faces. If such a photograph is copied twice, once correctly and then a second time from the wrong side of the film, so that a mirror-image results, we can cut both copies vertically along the median axis of the face and then combine the halves of the first picture with those halves of the second which are their mirror images. In this manner perfectly symmetrical pictures are obtained. In the case of most persons these pictures look surprisingly different from the original or from a normal photograph of the same person. Generally the objectively symmetrical pictures have a less "interesting," an "emptier," appearance than have normal photographs of the same person.[19]

Why is the fact that most faces lack perfect objective symmetry so seldom observed under normal conditions?

[18] So far as I know the German psychiatrist Hallervorden was the first to study these facts

[19] A third method consists in looking at the image of another person who stands before a mirror· In most cases the appearance of this mirror-image is strikingly different from the appearance of the same face as we usually see it. This is due to the fact that in the mirror the right half of the face, for instance, becomes a left half, and vice versa. Since the two halves are as a rule objectively different this leads to a remarkable change in the phenomenal character of the face. Many people do not seem to know that what they see in a mirror is by no means the face which other people see when they look at them.

Wertheimer's principle applies here: So long as a face is at least approximately symmetrical and, thus, sufficiently near a standard condition of clearness and simple regularity, organization of this face as a percept will tend to overcome such minor irregularities as exist objectively. With regard to symmetry it will have "too good" an appearance.

It would not be a valid objection, if we were told that the differences between the two halves of faces are probably too small for our apprehension; that our difference-threshold for differences of distances, sizes, and so forth, is too large. Actually our sensitivity for such differences is entirely sufficient; otherwise we could not recognize them so easily as soon as we assume an analytical attitude by which the eyes, the ears, or the outline of the cheeks are made more or less independent of the total pattern with its tendency toward symmetry. Moreover, those differences do not become altogether ineffective; because, although we are for the most part not aware of them as of deviations from strict symmetry, we do realize that the perfectly symmetrical photograph is not a true picture of the person in question, and that it shows less "expression" than a normal photograph of the same individual. A bold theorist might venture the hypothesis that, when irregularities of a percept are overcome by the tendency toward simple clearness of shape, there remains the stress by which such symmetry is maintained; and that it is this factor which gives a face its lively, interesting appearance.

With this explanation we are once more led into the domain of natural science. As a matter of fact, macroscopic physical states show exactly the same tendency. Ernst Mach not only made the observation that such states tend to develop in the direction of maximal regularity and sim-

plicity; he also gave an explanation in which he derived this tendency from elementary principles of physics. Macroscopic states tend to become as *stable* as possible, and in regular macroscopic distributions forces balance each other more fully than they do in irregular patterns; in other words, macroscopic regularity is associated with macroscopic stability. Macroscopic physical processes will therefore assume simple and regular forms rather than complicated, irregular distributions. When Wertheimer formulated his principle in psychology I happened to be studying the general characteristics of macroscopic physical states, and thus I could not fail to see that it is the psychological equivalent of Mach's principle in physics.[20]

For this reason the theory of psychophysical function need not add a new hypothesis in order to explain Wertheimer's principle. Since all macroscopic physical states follow Mach's principle, the same will hold for cortical processes; and if perception is found to obey Wertheimer's law, such correspondence is no more than a general case of isomorphism between experience and its correlates. There is, however, a special reason why such facts are mentioned in a chapter on memory. Traces, we have said, are organized macroscopic states, just as are the psychophysical processes to which they owe their origin. It follows immediately, and I have stressed this point above, that traces are likely to undergo changes, not only because external causes will gradually alter their properties, but also because all macroscopic organized states tend to change themselves in the direction which Mach's principle

[20] W Kohler, *Die Physischen Gestalten*, etc, pp. 248 ff Just as perceptual organization, self-distribution in physics may become "too good." The meaning of this statement and the facts in question are explained on pp. 251 ff. of the same book.

indicates. We are thus in a position to test our assumptions about the nature of traces. Is there any evidence that in the course of time traces tend to assume a simpler and more regular organization?

For many years memory has been investigated by a method which was first invented by Ebbinghaus. The subjects learn series of nonsense syllables, and recall of this material is afterwards tested. The results are found to vary according to the conditions which obtain during learning and during the time between learning and recall. It has always been a puzzling fact, however, that subjects who have otherwise a good memory need so much learning and rehearsing until they master a series of, say, sixteen nonsense syllables. Now, observation shows quite clearly that most subjects learn such a series not as an aggregate of its individual members, but rather as something like a monotonous phonetic melody. This monotony is due to the fact that all these members belong to the same class. We have thus a case in which a given material approaches a standard condition, namely that of homogeneity. One might therefore suspect that it is so difficult to learn such a series because in the realm of traces the tendency toward increased homogeneity is likely to undo what the subject is trying to build up, i.e., memory of a series of individually characterized data. An increase of homogeneity would obviously be tantamount to the disappearance of those characteristics by which the members of the series are distinguished from each other, and the difficulty of retention and of precise recall would necessarily follow. This assumption seems to be verified by a recent investigation in which Dr. von Restorff was able to show that it is in fact the monotony of a series which makes it so hard to

recall its members.[21] Among many experiments which all had substantially the same result I shall mention only one which is particularly simple.

In a series of ten members the second (or the third) item was a two-place number while all the others were nonsense syllables. The series was read only once, and after an interval of ten minutes—which was filled with an indifferent activity—the subjects were asked to name as many of these members as they could. A few days later they learned another series in which the second (or the third) member was a syllable, while all the others were now numbers. There were 15 subjects, so that the maximum achievement which this group could obtain was in each series 15 correct reproductions of the "single" member and 135 of the others. Results were

for the first series:

9 reproductions of the "single," and 34 of the other members;

for the second series:

12 reproductions of the "single," and 24 of the other members.

Since in a series there are many more items of the second than of the first kind, we have to compare percentages rather than absolute results. When we do so we find that for the two series taken together

70% of the possible maximum for "single," and
22% of the maximum for the other members

were correctly recalled.—I repeat that this is one of many experiments all of which yielded the same result: Members that do not belong to the monotonous part of a series

21 H. von Restorff, *Ueber die Wirkung von Bereichsbildungen im Spurenfeld.* Psychol. Forsch. *18* (1933), pp. 299-312.

are far better retained and recalled than those which constitute the semi-uniform part of it.

The test was made, both with a number and then with a syllable as the "single" member, in order to prove that not one class of *material* is better retained than the other, but actually the member which stands out against the monotony of the rest, irrespective of its class.[22]

For a moment one might believe that the explanation of this experiment is quite trivial· The "single" member of a series is so different from the others that in memory it will not easily be confused and distorted by their presence. On the other hand, such confusion is likely to occur among the members of the uniform group, so that here more incorrect reproductions will ensue. In fact, mutual contamination among members of the same class does occur. It was, however, shown in a further experiment that this is not the decisive factor in determining the striking asymmetry of results. The procedure was this. If $A_1BA_2A_3$... and $B_1AB_2B_3$... symbolize the two series which have just been mentioned, another series ABCD... was constructed in which all members differed at least as widely from each other, as A differed from B in the first two series. "Confusion" between A, B, C, D, and so on, can not occur, if the member C, for instance, is the sign $, D a button, and so forth, while A and B are a number and a syllable. From the point of view of the proposed explanation A and B ought therefore in this series to be quite as well reproduced as they were as "single" members of the first two series. If, however, it is mainly the organization of such series on which the retention of their individual items depends, we come to another conclusion. In the new series A and B, the syllable and the number, are no longer set apart as "single" members. On a lower level of similarity the new series is an approximately even course of events in which no member plays an outstanding rôle; and therefore both A and B will

22 Other experiments of von Restorff's prove that it is not the place of "single" members in the series on which this effect depends.

now be under the same stress toward homogeneity as are other members. From this point of view A and B should be less well reproduced in the new series than they were as "single" members of the previous series. Two experiments in which this question was tested decided in favor of the second alternative: A and B yielded less accurate results in the series ABCD ... than they had done as "single" members of the series $A_1BA_2A_3$ and $B_1AB_2B_3$.... Our interpretation in terms of organization is therefore verified.[23] [24]

The analogy of these facts to the tendency toward simplified organization in perception seems fairly obvious. Just as in our example the difference between the two halves of a face is suppressed, because thereby the face assumes a more regular and symmetrical appearance, so traces tend to lose their individual characteristics, if thus a standard condition of homogeneity is approximated. Precisely this agreement, however, may lead to an objection: Why assume that Wertheimer's principle applies both to the realm of traces and to perception? Not only memory, but also processes of perception and apprehension are involved in the experiments which I have described. When the subjects apprehend a series of the type $A_1BA_2A_3$..., they will be more strongly impressed by B as an outstanding member of the series than they are by the other items, which will even in perception tend to become a monotonous background. For this reason the trace of B will, from

23 A more detailed description of these and other experiments of von Restorff's will be found in Koffka's *Principles of Gestalt Psychology*, pp. 481-493

24 Professor C. Hull has recently given a set of axioms from which he deduces the inhibitions which occur in a series (C L. Hull, *The Conflicting Psychologies of Learning—a Way Out.* Psychol Review *42* (1935), pp. 491-516) It seems to me that his axioms will have to be changed if they are to account for the influence which in such experiments specific organization has on retention.

the very beginning, be a more stable and a more characteristic entity than the other traces can be expected to be. No wonder, then, that B is more frequently and more accurately reproduced.

There is a flaw in this argument; the series is a *sequence* of events, and in this sequence B as its second or third item cannot play any particular rôle until it has become a trace. Nevertheless it seemed advisable to vary the experiment so as to cause all members of the series to be apprehended under the same conditions.

For this purpose the following procedure was chosen. We showed a group of subjects a short series of five items only. The second member was a meaningless *figure,* the others were two-place numbers. Immediately after the presentation of this material it was reproduced in writing and drawing. Then followed a period of ten minutes during which the experimenter lectured on some problem of general psychology. Afterwards a second series of five items was shown and immediately reproduced, which had precisely the same structure as the first. Again the experimenter lectured for ten minutes, and then a third series followed the second member of which was a *syllable* instead of a figure. Altogether eight such short series were given, between which periods of lecturing were interpolated. The second member was a *syllable* in series no. 3 and 5, a *figure* in nos. 1, 2, 4, 6, 7, 8. A quarter of an hour after the last series had been shown and tested the subjects were unexpectedly asked to reproduce whatever they remembered *of all the eight series.*—I have to add that to a second group of subjects the same procedure was applied, with this difference, however, that in series no. 3 and 5 the second member was now a figure, while in all

the other series it was a syllable. It will help the reader to understand the experiment if I present its course in a simple symbolic form:

1.	2.	3.	4.	5.	6.	7.	8.
A	A	A	A	A	A	A	A
B	B	C	B	C	B	B	B
A	A	A	A	A	A	A	A
A	A	A	A	A	A	A	A
A	A	A	A	A	A	A	A

All A are numbers, all B are figures, and the C are syllables. The vertical columns represent the 8 short series which were given successively, separated by periods of lecturing. —To the second group of subjects the same scheme applies, except that now 6 C take the places of the B, and 2 B the places of the C.

The purpose of this experimental scheme is easily explained. The experimenter is interested merely in the final reproduction of figures and syllables; the only function of the numbers is to hide the intention of the experiment, i.e., to make the subjects believe that reproduction after each short series is a real test, after which they need not bother about the series. The syllables and figures, however, may be considered as constituting a sequence of their own whose members are separated by intervals of about ten minutes. (*Cf.* the second horizontal of the scheme.) Each member is in its short series apprehended as standing out against a background of numbers; there can be no doubt about its adequate apprehension; its immediate recall is an easy task. And yet, since in the sequence of figures and syllables there are six items of one class and only two of the other, the latter play, at least comparatively, the rôle of "single" members, while the former con-

stitute a more uniform mass.[25] The fact that for one group of subjects the "single" items are figures, while for the other group they are syllables, enables us to compare the results independently of the material which is given either "singly" or "uniformly."

The following tables contain (in terms of correct reproductions) the results which Dr. I. Muller obtained with this method.[26] In the second experiment (Table II) the interval between two short series was five instead of ten minutes, and the "single" items were given in series no. 3 and 6; otherwise all essential conditions were the same as in the first experiment.

Table I.

	1. Group (10 subjects)	2. Group (10 subjects)
Syllables	"Single" 17 out of 20 = 85%	"Uniform" 30 out of 60 = 50%
Figures	"Uniform" 38 out of 60 = 63%	"Single" 18 out of 20 = 90%

Table II.

	1. Group (20 subjects)	2. Group (20 subjects)
Syllables	"Single" 31 out of 40 = 78%	"Uniform" 51 out of 120 = 45%
Figures	"Uniform" 70 out of 120 = 58%	"Single" 38 out of 40 = 95%

[25] For several reasons it did not seem advisable to give only one "single" member among a larger uniform sequence of others

[26] I Muller, *Zur Analyse der Retentionsstorung durch Haufung.* Psychol. Forsch. 22 (1937), pp. 180-210.

Group experiments like these yield differences of results between two constellations; they do not give us a measure of dispersion for the numbers which we compare and, thus, no simple quantitative index for the reliability of their differences. In the present case we are nevertheless quite safe in concluding that both experiments confirm our expectation. In both tables there are four possibilities of comparing the constellation "Single" with the constellation "Uniform": two for a given material, either figures or syllables, i.e., between one group of subjects and the other, and again two within one group of subjects, i.e., irrespective of material. This gives us eight comparisons altogether. All of them show that reproduction of "single" members is superior to reproduction within a uniform class. It seems at first surprising that this effect of organization should be demonstrable in the realm of traces when the sequence in question extends over more than half an hour (in the second experiment) or even over more than an hour (in the first), and when so much time passes between one member of the sequence and its successor. But the fact is certainly not in contradiction with a theory according to which an hour must be the very tiniest stretch in the "dimension of past time."

Another proof that the principle of Mach and Wertheimer applies in the realm of traces has been given by Dr. von Lauenstein, not, however, in experiments on memory proper but in an investigation of successive comparison, a topic which we shall presently discuss for another reason. His results indicate that when an otherwise strictly homogeneous experience is interrupted by short stretches of another character this disturbance of complete uniformity

is to some extent evened out in the realm of traces.[27]

So far then our expectation has been fulfilled. One might only wish to see the principle clearly verified not only in cases where its effect lies in the direction of simple uniformity, but also in the case of more interesting standards.

It has been reported that figures which a subject has once inspected are afterwards reproduced with such errors as Wertheimer's principle would make one expect.[28] But other experiments on the same problem have been interpreted as giving no such evidence. Under these circumstances it might be doubted whether the experimental procedure has been altogether satisfactory. The subjects are shown not one but several figures. From this a certain ambiguity arises. In the subject's experience an individual figure is not strictly separated from the others: Neither is the group of figures as such a sum of mutually independent entities, nor is it plausible to assume that their traces constitute such a sum.[29] We are therefore not quite justified in interpreting the results of such investigations as though they told us how the trace of each figure changes separately. As it happens, some authors find convincing proof of the fact that under such conditions the figures influence each other. It seems therefore obvious that in the analysis of results much more attention should be given to alterations in the organization of the group as such. Otherwise the changes of its individual members may not be correctly understood, and changes in the organization of the group will easily be altogether overlooked. If, however, such changes are to be investigated the group should have a structure that approximates some particular standard, in relation to which they could be unambiguously interpreted.

A further difficulty is that subjects who are asked to

[27] O. Lauenstein, *Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler* Psychol. Forch *17* (1932), pp. 130-177.

[28] F. Wulf, *Ueber die Veranderung von Vorstellungen (Gedachtnis und Gestalt)* Psychol. Forsch. *1* (1922), pp. 333 ff.

[29] *Cf.* the experiments reported on pp. 257 ff., this chapter.

inspect a number of figures will naturally expect a later test and thus combine the inspection with various intellectual operations such as subsumption under a class, analysis, comparison with other figures, and so forth. Of such operations, too, traces will be left which may be quite as essential for later recall as the purely visual character of the figures. To some degree this difficulty could be avoided if the subjects became acquainted with the figures under circumstances in which they see them clearly without knowing about a later test, and thus without having any incentive for further elaboration.

One more test of the theory of traces should be mentioned. In discussing it we shall be led back to the main issue of our investigation.

At the end of the last chapter I explained von Lauenstein's hypothesis according to which the psychophysical correlates of certain relations are gradients. They extend from the correlate of one perceptual object to the correlate of a second object; their direction corresponds with the direction of the difference which we experience when attending to the objects, their slope represents the degree of this difference. We concluded that on this assumption our awareness of differences must depend on the distance by which the objects are separated; and we saw that on this point observation agrees with theoretical expectation.

The two following experiments are variations on the same theme. In Miss Jacobs' investigation two objects of slightly different brightness were exposed one on each side of a fixation-mark. When the distance between these objects was increased from 10 through 80 to 200 cm., the just noticeable brightness difference was found to grow regularly, just as Lauenstein's hypothesis would demand (*Cf.* ch. 6, pp. 231 f.). Dr. Kleinbub repeated the experiments

with only this change that the objects were shown *successively,* the second being presented a few seconds after the first had disappeared.[30]

For a group of observers the average noticeable differences were now, for the distances of

10	80	200 cm
8.6	17.4	33.9,

all measured in the same arbitrary unit. The first impression is that these figures merely corroborate Miss Jacobs' results. That they have still another significance will become apparent when we consider a further experiment in which the subjects, instead of fixating a mark between the objects, turned the eye from the first to the second object as one appeared after the other. Under such circumstances the subjects are, of course, just as fully aware of the distances between the objects as they are when fixating a stationary mark in the middle. And yet in this case the just noticeable differences were, for the distances

10	80	200 cm
6.6	5.8	6.2

If these figures are compared with those of the last experiment, or with those given previously (on p. 232), there can be only one interpretation: When, in comparing two brightnesses, the subjects fixate first one and then the other object, the distance between these objects has no influence upon the just noticeable difference. The threshold remains constant and is very low. (This result has

[30] M. Kleinbub, *Ueber die Unterschiedsschwelle fur Helligkeiten bei verschiedenen Abstanden der Vergleichsobjekte und Fixationswechsel.* Diss. Berlin (1933).

been confirmed in a further experiment under slightly different conditions.)

I shall now give the explanation which follows from the theory of traces. There is no fundamental psychological difference between a relation which is apprehended when the objects or terms in question are both simultaneously present and a relation of which we are aware in a succession of objects. Von Lauenstein was therefore right when he assumed that the psychophysical counterpart is essentially the same in both cases: When the objects are simultaneously presented, the gradient extends from the correlate of one object to the correlate of the other; when they appear successively, it extends from the trace of the first to the actual correlate of the second. For this to be possible we need only assume that the trace is a sufficiently adequate representative of the process by which it has been formed, and that therefore a gradient may develop between the trace of the first and the process of the second object, just as it develops between two simultaneous processes. With this premise the first of Kleinbub's results will be easily understood if we add our previous assumption, that traces are immediate products of psychophysical processes, and that, as such, they have the same cortical location as have these processes themselves. In this case, variation of the distance between the two successively given objects means *ipso facto* corresponding variation of the distance between the trace of the first and the process of the second object. Thus the same reasoning applies to the gradient between the trace and the process as had previously been applied to the gradient between two processes: With increasing distance between the objects the gradient will become less steep, because in this experiment the dis-

tance between the trace of the first and the process of the second object varies with that objective distance. The conclusion is verified by the experiment. It seems to be true, therefore, that traces have the same cortical location as have the processes by which they are produced.

Although apparently quite different, the outcome of the second experiment actually confirms these assumptions. When the two objects are fixated one after the other, the images of both are successively projected on the fovea. Pathological evidence shows that a definite circumscribed region of the striate area corresponds to the fovea of the eye. In this region therefore the first process forms its trace; and after a short interval the second process arrives in the same region. This is true whatever the distances over which the eye travels from one object to the other. Consequently there is no variation of distances between the trace of the first and the process of the second object when in the last experiment the distance between the objects is increased. The trace and the process are always immediate neighbors, and, if our assumptions are correct, the noticeable difference must remain constant and very low. This is exactly what Kleinbub's data prove.

It is to be hoped that, when in the future such experiments will be repeated, a greater time interval between the presentation of the first and that of the second object will be chosen. The trace of the first object would then be older, it would be more assuredly a trace, when the second object is presented. Under these conditions all noticeable differences may grow considerably; nevertheless it is quite possible that variation of the distance between the objects will still have the same effect on the just noticeable differences in one case, and the same lack of any effect in the other case, as it had in Kleinbub's two experiments.

III

The existence and the properties of traces have not been discussed here merely because traces as such are entities of considerable interest. For our purpose they assume a more particular importance by the fact that some contexts extend beyond the realm of phenomena into that of traces, and that, vice versa, certain experiences are felt to belong to contexts the main part of which lies "beyond." We shall soon see that requiredness is a characteristic of some such amphibian contexts. Thus the realm of traces appears in a new light: Most remnants may be "mere facts"; but others participate in the constitution of particular contexts within which certain things are "right," while others are "wrong." Now traces are generally referred to as exceedingly remote and not yet quite serious postulates of psychological thinking, which for perhaps centuries to come science will remain unable to investigate. If requiredness were found to be in touch with such nondescript entities, the connection could not mean very much for requiredness itself. That it actually means a great deal could not become apparent until traces had been shown to be quite in keeping with other concepts of science and altogether susceptible of concrete exploration. For this reason a short description both of trace theory and of such investigations has been given in this chapter.

I will now explain more clearly how requiredness comes in touch with the concept of traces. I have just stated that the extension of some contexts is not restricted to the phenomenal world. A simple example of this fact is successive comparison, the psychological process by which, for instance, just noticeable differences are found in experiments such as those of Kleinbub. In successive com-

parison we recognize a relation which obtains between two objects given at different times. It stands to reason that there can be no relation, unless there be at least two terms of which the relation is predicated. At the time of comparing the subject must therefore have two such terms. But, since in successive comparison the first impression itself is a matter of the past, it was once generally believed that successive comparison must actually be a simultaneous comparison of the second object with a memory image of the first impression. In a way psychological observation has destroyed this conviction. When comparing two objects or events which are given successively we do not, as a rule, produce a memory image of the first when the second appears. It would not even help us very much if we did, because, when the difference between the impressions is small, the accuracy of actual comparison may still be great, while under the same conditions an actual memory image will hardly be a sufficiently adequate copy of the first impression. For this reason the memory image is rather likely to *disturb* the process of comparison.[31]

On the other hand successive comparison is, of course, no less compatible with logic than are other psychological processes. It belongs, in fact, to the meaning of any real comparison that there are two things to which comparison refers. *In concreto* three different things may happen when we compare pairs of objects which are given in succession. First, we may make use of memory images; but as a rule we do not do so.—A second situation arises whenever subjects give many judgments about impressions which all belong to a certain region of a common scale, e.g., of weights or of brightnesses, of visual sizes, and so forth.

[31] *Cf* F Angell and H. Harwood *Amer. Jour. of Psychology 11*, pp. 67 ff. 1899. D. Katz, *Zeitschr. f. Psychol 42*, pp. 302 ff and 414 ff. 1906.

After a while even a *single* impression which belongs to the same scale will appear as heavy or light, as dark or bright, as big or small. Although there is no longer any conscious reference to a standard, such a standard has been established—obviously in the realm of traces,—and the apparently "absolute" judgment depends on the level of this standard. If we change the range from which the objects are taken, the "absolute" judgment will gradually shift in correspondence with that change. From a phenomenological point of view, however, no such "absolute" judgment involves actual comparison; only *functionally* its nature is still "relative."

Besides these two there is a third possibility which, I believe, is realized under normal conditions of simple successive comparison: We lift, for instance, one weight and soon afterwards a second. The second action has hardly begun when we are ready to say "the second is heavier" (or "lighter"). No memory image of any accuracy may be present; and yet the judgment may be both subjectively sure and objectively correct. Moreover, from the phenomenological standpoint it may be a genuine judgment of comparison even in the absence of a memory image. For, if the second impression is not referred to such an image, it is nevertheless clearly referred to *something*. This "something" is phenomenally located in the immediate past; from there the reference is felt to emerge and to give the present object its truly relative character of being heavier or lighter. I use just this expression according to which the reference issues from the first object, because in such cases the second impression is not at first an independent item to which then a reference backward is added; rather it *appears* with a character of relatedness, it usually *appears* as heavier or as lighter.

To repeat: Although in successive comparison there is such a reference, the other term remains, as a rule, "outside" at least to this extent that its specific properties are not phenomenally given. We know there is that definite thing, so much is implied in the reference; and, besides, the accuracy of our judgment confirms this knowledge; but as a concrete entity which makes such accuracy possible the first object is for the most part hidden. We have thus come back to a previous discussion. Relations which extend from events in the recent past to other events in the present are, under the circumstances of simple successive comparison, examples of "transcendence." It is customary to discuss this notion as though only from the phenomenal realm transcendence could perhaps be found to extend into another realm beyond. In successive comparison it seems to be rather the other realm from which a relational trait extends into the world of direct experience.[32] As to the nature of the hidden term to which the present term is felt to be related there cannot be much vacillation. Just as a memory trace it must be a more or less adequate representative of the process with which the first impression has been associated. Nobody will assume that after an experience one trace is left which participates in successive comparison, while another trace of the same event makes facts of memory possible. The obvious view is of course that the same remnant operates in both cases. In successive comparison this trace is then the thing "beyond" with which the present event is felt to be in reference. In terms of cortical theory this reference has its counterpart in a gradient which extends from the trace to the present process.

[32] W. Kohler, *Zur Theorie des Sukzessivvergleichs und der Zeitfehler.* Psychol. Forsch. *4* (1923), pp. 121 f.

In our experience of some phenomenal contexts there is perhaps no indication of any transcendence whatsoever. It may be quite easy to prove that the actual properties of these contexts are codetermined by conditions which have been established in the past; and yet such determination need not as such be revealed in immediate awareness. It is one thing to infer the existence of extraphenomenal conditions; it is quite another thing to be directly aware of their presence.

Again, contexts may remain nothing but traces for long periods. Or, if they contribute to present processes, it is only the result of their influence of which we are aware, not the action as such by which this result is attained.

But a third class of contexts lies partly in the realm of phenomena, partly in that of traces; and their relational structure is felt to extend from actual experience into a realm "beyond," or vice versa. As a first example of such transcendence a case of imminent recall was described in the fourth chapter. Successive comparison would have been quite as appropriate for a demonstration of such amphibian contexts.

If we speak of "amphibian" contexts we do it from a phenomenological point of view. In the psychophysical domain there is of course no reason at all why traces should not participate in contexts of actual cortical function. Our assumptions allow present processes to extend freely into the realm of traces. More particularly: To the *immediate* past, present processes will almost always be related, because this past belongs just as much to their functional neighborhood as does their immediate *spatial* environment.

It comes then to this: Transcendence means that for some reason not all members of a given cortical context

are able to express themselves in phenomenal language; and that yet the presence of such silent members with quite specific characteristics is clearly and directly implied in what the other members of the context have to say.

It is by no means a rare occurrence that contexts extend in time, and that behind the present phase of such contexts their earlier parts fade out of phenomenal existence. Even then the present development often follows a scheme the general outline of which has been established in the recent past and is now gradually being completed. Thus a certain degree of coherence and of consistency becomes possible in mental life. In speaking, for instance, we end a sentence with certain words, with words in one or the other grammatical form; referring backward we use the plural or the singular of a pronoun—all this according to the manner in which the sentence has begun. And yet this beginning may no longer be given in the form of phenomenal contents. In reading we understand the later parts of a sentence in relation to previous parts, of which, as a rule, little or nothing remains as an actual distinct experience. Again, it takes several notes to establish a musical key. When we sing, however, we proceed from one note to the next as the key demands, although besides the present pitch no more than one previous note may be phenomenally given as an actual "image."

It is at this point that requiredness comes in touch with the concept of traces. In successive comparison, it is true, transcending reference is "indifferent," inasmuch as it does not seem to matter whether the second weight is heavier or lighter, the second object brighter or darker, and so on. Apparently such a primitive reference develops

neutrally between any two phenomenal data. This may not always be so; but it is true in countless cases. On the other hand when we think, speak and read, when we sing or when we hear others singing and playing, then the situation is for the most part completely changed. One word is now "wrong," the other "right"; it "has to be" the plural, not the singular; one intonation is just a trifle "too low," but then it shifts to precisely the "right" pitch.

All these are instances of requiredness; of requiredness, however, that appears under remarkable conditions. The first part of the sentence may still be felt to hover in the background, but its concrete and specific content is no longer phenomenally there. *Mutatis mutandis* the same could be said when, as singers or listeners, we follow the course of a melody. And yet requiredness may at the same time be quite definite; it may even be most fastidious about the further completion of the contexts in question. Phenomenally, we know, no conditions are given in these cases from which demands of such strictness could issue; they must come from the recent past which is now hidden. And this is precisely the source from which they are actually felt to come. We are thus led to conclude that in such instances demanding vectors issue from incomplete contexts of traces, just as in other cases they issue from incomplete contexts *within* the phenomenal realm. Thus, among the relational traits which in amphibian contexts extend from traces into the phenomenal present requiredness occurs not infrequently.

When we look back upon our previous discussion of transcendence (ch. 4) we find that the same reasoning applies to the example which was then analyzed: I know that I know the name which I heard mentioned yesterday, although I do not at once succeed in actually recalling it.

Eager as I am to find it, I try several names in succession. Now, on phenomenal grounds I cannot decide whether these names are right or wrong, because here I have not yet the standard with which to compare. "Outside," however, the standard is present. And as I try name after name, the standard replies from beyond: "wrong"—"a trifle better"—"not yet quite"—"there, perfect." Once more we have an amphibian context; requiredness is its most striking trait; and the vector, which in this context objects to one thing or readily accepts another, owes its character as a quite specific demand entirely to the properties of a trace.

I am inclined to regard this example as even more convincing than those others, in which the immediate past insists on being completed in a certain way; for in this instance there simply is no phenomenal memory image from which the demand might issue. If there were such an image, I should not try so hard to *find* the name in question.

It is true that under these circumstances the 'I' or the 'self' is interested in finding the right name. But let us not be deceived by this subjective phase of the situation which is quite irrelevant to the essential point of our argument. It is not the I primarily which says "yes" or "no," as one name is tried after another. I have no phenomenal basis for either of these judgments. Acceptance or rejection comes distinctly from an authority "beyond"; and the self can only ratify what this other agent really decides to say. Let us remember how often in the phenomenal realm demands are felt to issue not from ourselves, but from 'objective' components of our situations; and that, even when the self is strongly interested, the situation may contain 'objective' demands. To be sure, now we take one

further step, inasmuch as in the present instance the self has not even an adequate view of the object, from which acceptance or rejection issues. But once it is realized that requiredness has no necessary connection with phenomenal 'subjectivity,' this further step will be more easily taken.

As the situation of imminent recall was just now described, the nature of the trace determines the nature of the demanding vector to which I expose various names in a tentative way. Almost the same situation may, however, be seen in another light: I know in what context I have heard the name in question. And I know also that the thing "beyond," which I am not yet able actually to recall, is precisely the name that fits the given context. In other words, I know that the entity "outside" is just the completion which the given context requires. There is no image or anything else in the phenomenal field which by its specific properties fulfills this demand; otherwise I should not be so eager to remember, i.e., to produce such a phenomenal datum. The missing part is still "beyond"; its specific properties are hidden. If nevertheless I know that it fits, that it is the "right" completion, this fact can only have one interpretation: Requiredness extends from the incomplete phenomenal context into the hidden past, and there it accepts one particular entity, the properties of which make it the fitting completion of the context, before such properties are represented in the phenomenal realm. From the point of view of physiological theory this entity is a trace.

The concept of traces which has been introduced in this chapter refers to purely physical entities. We are thus forced to conclude, in the first place, that specific demands both of acceptance and of rejection may issue from certain physical entities; and in the second place, that such physi-

cal entities may fit into given phenomenal contexts because they have the properties which these contexts require for their completion. It is a curious question how anything like requiredness can emanate from physical entities in the cortex. But it is no less remarkable a problem how such cortical entities can be "right" or "wrong" in certain contexts. Are we not all convinced that physical entities are *mere* indifferent facts for which nothing else is either "right" or "wrong," and which can not themselves be "right" or "wrong"?

CHAPTER VIII

A Discussion of Organic Fitness

I

Requiredness does not, as such, occur among the data of physiological observation. When the heart contracts the blood is driven into both the arteries and the lungs. This is a mere fact, analogous to similar facts which we study in physics. We speak of causation in such cases, but not of demands. Again, when peripheral blood vessels like those of the limbs contract, more blood streams naturally into the head. This is a simple case of causation, complete in itself. Nothing like requiredness is observed to participate in this sequence of events; nor is there any place left for it, since under the conditions nothing can happen other than what actually happens.

On the other hand, if the same facts are considered in a larger context, they assume in our thinking a new aspect of "fitness." Unless circulation in general is steadily maintained all the usual activities of the organism will quickly disappear; some tissue will begin to disintegrate in a few minutes, the rest more slowly. Thus circulation appears to us as required in the context of an individual's life. In our other example, when do the vessels of the limbs contract, so that more blood streams into the head? This happens just when for some other reason the blood pressure has fallen in the arteries of the head. Now the brain

belongs to those parts of the organism in which irreparable damage follows insufficient circulation particularly quickly. What really occurs seems to us therefore required for the salvation of an important organ and, thus, for normal life.

It is, of course, by no means remarkable that some physical events "fit" certain subjective interests. After a few weeks without rain the farmer welcomes a storm as just the thing needed. Yet many farmers will hesitate to believe that their wishes have any influence on the actual course of the weather; they will also doubt whether anything but causal physical relations determines what is happening in the atmosphere. Similarly, if somebody's interest is directed toward the continuance of life in a given organism, the mere fact that *some* events in this organism are apt to prolong its life cannot constitute a problem. Such a coincidence between an interest and causally determined events may occur by mere chance, just as rain will sometimes fall in chance coincidence with the farmer's wishes.

A serious problem is, however, presented by the observation that not only a few inner activities of the organism, but almost all such functions seem normally to have the same property: they occur, and they also vary from one moment to another, in such a manner that the life of the organism may be preserved for a long time. Under certain adverse conditions, it is true, all variations of which those inner functions are capable seem unable to prevent unfortunate developments; and sooner or later all multicellular organisms die. But certainly so many organic events do constantly operate in a direction by which the life of the organism is prolonged that no sane biologist has ever denied the urgency of this problem: Why does organic function in all its bewildering complexity take, for the

most part, just the course by which a standard and stable condition of the organism is safeguarded or, if necessary, reëstablished? The continuance of life, in the case of most men clearly a matter of personal value, seems at the same time to constitute a value for the physical organism itself. And yet, if there is no generic difference between other organic events and our two examples—the circulation in general and a simple redistribution of blood pressure, all particular events in the organism have their particular sets of causes by which they are strictly determined. They have to occur precisely as they do, and no choice is left in which a value or a demand might decide what happens.

Many theorists believe with Descartes that this problem of "fitness" or of "causal harmony" in the organic realm will find a satisfactory solution if we assume that the organism is a *machine*. In machines, just as in the organism, mere causation leads to results which satisfy certain human wishes. But who would, for this reason, attribute any such wishes to the machines themselves? In some machines there are even functions by which their own existence is protected, as when in automobiles the water of the radiator is made to circulate and to be cooled. It seems at first a plausible idea that with a gradual increase in the number of such devices machines would become not only more complicated but also immensely more "fitting." At the same time this "fitting" nature of their functions would be due to merely causal factors, just as it is in simpler machines.

If this notion could be successfully applied to the organism, the problem of causal harmony would indeed lose much of its puzzling character. One might object, it is true, that in this manner the problem is merely shifted from one field of biology to another: Nobody has ever

constructed an organism. Thus a machine theory of life involves much confidence in the processes of *evolution.* Whenever we explain a case of organic "fitness" by the assumption of a corresponding device in the organism we present thereby an additional problem to those who try to explain phylogenetic development, and who cannot yet satisfactorily do it in a single instance. Whether or not organisms are actually machines, however, their amazing anatomical complexity is a fact which must under all circumstances be interpreted by the students of phylogenesis. Thus, if these biologists have a difficult task, they seem to have the same task under all circumstances; it does not seem to be more difficult if "fitness" is explained by a machine theory of life than if such fitness is attributed to unknown causes.

Generations of biologists have been dominated by this particular conception of the organism. Most physiologists and zoologists are so now, and in the most valuable work on "fitting" physiological processes which we have, Cannon's book, causal harmony is consistently attributed to the existence of special devices which tend automatically to maintain such harmony after they have once been installed by evolution.[1] Nor is the influence of these ideas confined to those who actually use them in their explanation of organic "fitness." Some biologists and philosophers, the Vitalists, are thoroughly convinced that no complication of merely physical causes whatsoever can lead to just that causal harmony which we observe in organisms.[2] But when they consider how the very utmost of such "fit-

1 W. B. Cannon, *The Wisdom of the Body,* 1932, p 285 f.

2 The Vitalists to whom I refer here, assume that a special agent is responsible for the causal harmony of organic events. From this school we have to distinguish that of "Emergent Vitalism" in which entirely different views are held.

ness" could possibly be achieved by purely physical means, they invariably begin to think in terms of machine arrangements, precisely as do most of their opponents, the "Mechanists."

There are more reasons for regarding organisms as machines than the fact that machines work "fittingly" while all their operations are causally determined. A great many anatomical data seem to show directly that the organs of the living system are devices which make "fitting" functions physically necessary. Such is the structure of the eye, that of the heart with its valves, of the lungs, the kidneys, the nerves, the muscles, and so forth. Apparently these organs also *operate* as though they were devices in a machine which differs from our engines merely by its amazing complexity and much more subtle "fitness." It seems fair to generalize and to say: When for every function a corresponding device will be found in the organism, and when every particular "fitness" of organic events will be shown to follow causally from those devices and from their adequate connection, then life will be entirely understood. If any "fitness" is left which cannot be thus explained, then, the Machine Theorists would say, definite limits of natural science will have to be acknowledged. And the Vitalists would heartily agree. I am not the first to observe that, after all, both parties have a great deal in common. We shall next inspect this common ground more closely.

What exactly is a machine? As a rule neither Machine Theorists nor Vitalists give us a clear definition of this important term. What is the principle that can explain organic "fitness" according to the first school, while it is incapable of giving this explanation according to the second?

The more general concept under which that of a ma-

chine may be subsumed is that of a physical system. In a given system a great many different processes are generally possible. According to the circumstances which happen to obtain, one or another of these possibilities will be realized. If, for instance, a number of masses can move freely under the influence of mutual attraction, their actual movements depend upon their initial positions and velocities, and will vary with these. Such variability or freedom can, however, be limited: Suppose that two of these masses are rigidly connected in a way which the forces of the system are unable to alter. Whatever initial conditions may now be given, all those possibilities of movement are henceforth eliminated by which the distance of these two masses would be either increased or shortened. Similarly more such unalterable conditions, or "constraints," may be introduced. If this happens the freedom of dynamic action, the range of possible processes, will be still more reduced.

A simple example will make this clearer. Through a rigid sphere we bore a hole, and into this hole we insert an axle that just fits into the circular opening. If the axle is somehow fixed on two solid supports, how many different movements can the sphere perform? Only two. It can either move along the axle or turn around it. We need not count such other movements as consist of locomotion and of turning at the same time; because, as this very description implies, they may be considered as combinations of those two elementary forms of movement. Where, as is here the case, a system has two elementary possibilities of function the physicist speaks of "two degrees of freedom." In our present system only *one* degree of freedom would be left, if on the axle there were two protruding parts, one on each side of the sphere and in con-

tact with it. Because of this further constraint the sphere could no longer move along the axle; it could only turn around it.

To a large extent we can choose the constraints of a system as we wish. Thus we are enabled to build systems whose function "fits" our purposes. Such systems we call machines. Typically a machine has only one degree of freedom. If it operates at all, *one* function is prescribed for all its parts by its construction. Such a system need not be simple. A great many of the machines which are operating in our factories are enormously complicated arrangements. And yet they have only one way of functioning. Whatever be the driving forces which make them turn and move, the manner in which all their various parts function is once and for all prescribed. It is not determined by the nature of those forces, but by the permanent structural arrangements or constraints which keep our machine systems on a "fitting" and useful one-way track. The principle, therefore, that gives machines a "fitting" mode of operation resides entirely in their structural make-up. According to this principle arrangements, devices, constraints make for "fitness." The dynamics of a system becomes "fitting" merely because it is forced to become so by those structural factors.[3]

Not all systems which we call machines operate with a single degree of freedom. Steam-engines, for instance, which are equipped with a safety-valve have two degrees of freedom. Up to a certain pressure the pattern of func-

[3] By turning a wheel or moving a lever the human operator may alter the relative position of some structural parts, and thus vary the way in which his machine functions. Theoretically speaking, he thereby transforms one machine into a different second machine.—If the machine itself changes its own make-up under certain circumstances, we speak of regulation, a case that will be discussed in the next paragraph.

tion remains the same; but at the critical point the lid of the valve, which was stationary before, begins to move relative to its environment so that steam escapes. This means a second degree of freedom since the main operation of the machine occurs with or without this additional form of movement. A safety-valve is a regulating device or a regulator. Such regulators as occur in man-made machines contribute greatly to the "fitting" function of these systems. Apparently, however, this property is again due to the particular way in which they are built rather than to any new principle. It would not be difficult to construct a steam-engine in which at a critical high pressure a valve is not opened but closed. The change from the "fitting" to such an altogether suicidal type of function is merely a matter of the structure which we give to the system, not of the forces which make it move. To this extent the "fitness" of a regulating device follows from the same principle which gives machines in general a "fitting" mode of operation.

We seem now prepared to decide whether organic processes owe their causal harmony, their "fitness," to the same principle. At this point, however, some theorists would probably raise an objection: 'We admit,' they would say, 'that confronted with the "fitness" of organic function we like to call the organism a machine; because machines operate "fittingly," and yet according to principles of common causation. But it is not fair to take this comparison literally. We do not do so ourselves. If therefore you should find that organisms are actually not machines in this narrow sense, your argument will miss the mark. As we use the term, the word machine has a broader meaning.'

My answer would be: What is this broader meaning? Whatever else we may call a machine it is only in those

systems which I have just described as machines that "fitness" of operation seems immediately and clearly understood "on principles of common causation." Such "fitness" is brought about by devices, by the anatomy of the machine. If this characteristic of machines be eliminated, the concept either becomes useless in biological theory, or else we have to indicate by what other principle inanimate systems are made to operate "fittingly." I say "inanimate systems" because, when that distinguishing characteristic is sacrificed, the word machine seems to be no more than a synonym of this general term.

I insist upon this point partly because higher organisms *are* to some degree quite obviously machines. Organic function is subject to limiting and relatively permanent anatomical factors. The valves, for instance, which we find inserted into the circulatory system are certainly useful regulating devices. The physicist, it is true, will of course ascribe a great many, in fact countless, degrees of freedom to the organism. Our head has 3 degrees of freedom in its relation to the trunk, each hand 2 in relation to the forearm, and so forth. But even simple man-made machines, so we have seen, need not be restricted to a single mode of function. Any automatic regulation in such systems means one more degree of freedom. The great variety of possible functions in the organism may therefore simply mean that in the living machine there are far more regulating devices than any engineer has ever thought of in his boldest day-dreams.

This interpretation has the great advantage that it explains all organic "fitness" by one principle, namely that of appropriate anatomical devices. On the other hand, it becomes less plausible by the fact that such an explanation leads necessarily to a great many auxiliary hypotheses. We

know of some anatomical structures which, from the point of view of physics, are undoubtedly regulating devices. But even the most careful investigation of the organism fails to reveal that enormous number of particular arrangements which must be postulated if the machine interpretation is to be accepted as generally valid. It is entirely beyond our power to count all the variations of function of which the human organism is capable. With a given position of the limbs a host of different visual processes are compatible. When reading or writing a man can breathe through his nose or mainly through his mouth. When sitting in a chair he can assume one position or another while his heart beats at a practically constant rate. From the point of view of mere logic it may of course be possible to maintain the machine principle, and to interpret all such variations as due to and as controlled by the subtle play of many regulating devices. Particularly the nervous system, however, would in this manner become simply crowded with hypothetical structures of whose existence we have no evidence whatsoever.

Those, on the other hand, who find this perspective far from attractive, have only one other path to choose: The anatomical devices of which we actually know are quite unable to confine organic function to one track. A number of anatomical structures, one will readily admit, seem to operate in a way that resembles the function of regulators in machines. Thus certain degrees of freedom which the organism possesses may indeed be due to the play of regulating devices. However, where variability of function cannot be correlated with such devices, another assumption may easily appear as much more plausible than the machine theory. We remember that a system of masses may move freely in various ways even though *some* con-

straints impose limiting conditions on their displacements. Thus the existence even of many anatomical structures and devices in the living system is entirely compatible with a type of function that varies freely within the limits prescribed by those constraints. From this point of view it seems an artificial procedure to add more and more hypothetical regulators until each possible variation of function is explained by their play. Why not assume that, so far as no devices are discoverable in the organism, its function is as freely variable as are the processes in physical systems which do not belong to the machine type? As a matter of fact, we can make this assumption; but it involves a new task. In all its variability organic function remains normally "fitting." To the extent to which the existence of particular anatomical devices is admitted, these structures may *contribute* to such "fitting" operation. Inasmuch, however, as organic function is now said to vary freely within these limits, the problem of causal harmony is once more raised. With the new assumption one would implicitly refuse to accept a solution of this problem in terms of the machine principle. At least this principle would now no longer suffice for a complete explanation. Is there any factor besides constraining devices that could give processes a "fitting" direction?

II

As the last paragraphs will have shown, our problem centers more and more about the concept of regulation. Indeed, regulation means in biology just the fact that, so far as circumstances allow, a standard condition of the organism is preserved by a curious harmony among organic events; that, furthermore, if once disturbed, this

standard condition is soon reëstablished by an equally astounding fitness of correcting activities. Those who do not yet know with what obstinacy and precision a great variety of processes achieve such regulation will find Cannon's book highly instructive. To add only a few instances to those already mentioned: The osmotic pressure, the slightly alkaline reaction, the special chemical composition, the total volume of the blood and, *mutatis mutandis,* doubtless of the lymph, vary in man but little from certain normal conditions; if once they do assume abnormal values, correcting influences seem often almost frantically at work. And we know that greater deviations, mostly if of some duration, would lead to irreparable destruction and finally to death. The mere fact that human beings recover spontaneously from many a minor and often from a serious disease bears witness to a surprising regulating power in organic function.

Is such a regulation actually a matter of as many anatomical (or histological) devices as a consistent machine theory of life would have to assume? It seems to me that anyone who intends to defend such a theory will do so against tremendous odds.

As a matter of fact, in certain cases the situation is not merely that regulating structures are as yet unknown; on the contrary, we know that in these instances there are no such structures. In this connection one example will suffice, since it refers to a particularly important phase of life.—We learn more and more about the chemical complexity of the "internal environment," the blood and its derivative, the lymph, by which most tissues are surrounded and pervaded. Granting that circulation of the blood is as such maintained by devices, we find nevertheless that in most other respects no special arrangements

determine the behavior of this substance. Its various components mix and can freely react with each other, which is certainly not in line with the strict machine principle; they pass, moreover, with few exceptions freely through the walls of the capillaries into the tissue. Here they fill as lymph or tissue fluid "all the chinks and crannies of the body structure." [4] If the blood is at least confined to specific conductors, no such special arrangements exist for a fitting distribution of the lymph, excepting the lymphatics through which, after a time of complete freedom in the tissue, part of the lymph returns to the circulatory system. The lymph carries both the substances which yield the energy necessary for all kinds of processes and the material by which older material of organic structures is to be replaced. It also carries the waste products of cellular activity away to the blood stream. In the walls of cells, it is true, the existence of particular devices may be postulated by which a fitting selection is made from the chemicals that the lymph contains; but we cannot go very far in this direction without meeting a serious difficulty which we shall next discuss. In any case outside the cells, too, the lymph seems to distribute itself most fittingly; otherwise grave consequences would soon follow, when the locus of increased activity and of subsequent local demand shifts from one part of the tissue to another, and when there is any delay in the regulating innervation of the blood vessels. Some factor which cannot be a device seems to regulate such fitting distribution of this part of the "fluid matrix."

In one respect the organism differs essentially from all man-made machines. When describing these systems we are entirely justified in speaking of permanent structures which control function, but do not themselves depend

[4] W. B. Cannon, *Ibid.*, p. 28.

upon function for their existence. A rigid steel connection, it is true, may become "old" in the sense that its inner cohesion will gradually be affected when it moves for years in a machine; but at least its substance exists and remains the same independently of those operations which it controls. Not so, of course, the organs of the living system. In the organism the dependence of structure on various functions goes in fact so far that here the distinction between structure and function may sometimes almost appear as arbitrary. Whenever the organism is compared with a machine we should at once remember that no anatomical structure, not even a bone, consists permanently of the same material. Slowly and continually this material leaves the tissue to which it belonged, while other new material takes its place. After a while each organ is the same only inasmuch as its morphological characteristics are concerned; it is a new organ if we judge in terms of its material. This is indeed an essential point. In the organism two different kinds of function may be distinguished. One is function in the usual sense of the word, i.e., a set of comparatively rapid operations which, according to the machine theory of life, are kept fitting by specific devices, the organs. The second type of function operates more slowly; it steadily replaces the old material of the devices by new material. From this point of view an organ is itself a stationary process rather than a thing. Since the rate of the latter processes is, as a rule, much slower than that of the first type, and since the structure of the devices is kept stationary in spite of the change of their material, no objection ensues thus far against the notion that the rapid operations, the functions in the usual sense, are kept on fitting tracks by those devices or organs. But what about those other functions by which these

devices themselves are being permanently dissolved and yet most fittingly kept stationary at the same time? What factors are responsible for such fitness? If we assume that in this case again special devices provide for causal harmony, we are at once confronted by the same problem. These new devices which everywhere regulate the gradual replacement of the material of other devices will be themselves just as subject to metabolism as are those other organs. Consequently, if the metabolism maintains their adequate structure, we have again to ask why the metabolism should operate so fittingly in their case. A third set of devices? Obviously we are now in the well-known danger of a *regressus ad infinitum.* Devices must apparently be superadded to devices; if we wish to remain loyal to the view that organisms are machines, one fitness after another will have to be explained by further machine structures. And yet there will never be a satisfactory end, as one auxiliary hypothesis after the other makes the total system more and more complicated. In science this state of affairs is generally taken as a sign that something is fundamentally wrong with the basic premises of a theory. No doubt, the fact that in the living system all organs or devices are themselves stationary states to which the concept of regulation applies has not been sufficiently considered by those who regard organisms as machines.

The suspicion that at least some fitting organic functions are not due to machine arrangements has recently been strengthened by certain observations of A. Bethe and other physiologists. These authors showed that regulation may occur in cases for which evolution could not have prepared corresponding regulators. As an example we may take locomotion in spiders which have lost one or several of their eight legs. Normal locomotion, for in-

stance in a straight line, presupposes in these animals a complicated distribution of operations among the legs. From the point of view of a machine theory such a coordination must be effected by particular devices. This normal coordination becomes, however, unfitting after the loss of a single leg or a group of legs. Of such losses more than two hundred different variations are possible in the spider. It has not yet been observed what happens in *all* these cases. But in those which have been investigated a new and again a fitting coordination among the movements of the remaining legs was found to occur. It seems particularly important that no "learning" was implied in this remarkable regulation. When the number of legs had been reduced the new mode of cooperative function appeared at once.[5] By some stretch of the imagination one might equip the spider with regulating devices which would come into play after the loss of one leg here or there, and lead to new local innervations with a fitting total result. But there are too many possibilities if those cases are included in which more than one leg is lost. Few will believe that during evolution all these abnormal conditions have ocurred so often that regulating devices could develop for each case, and that such devices are now inherited by the spiders of our time.

It would not be fair to omit at this point those older experiments by which H. Driesch, and after him many others, demonstrated regulation in *morphogenesis,* the development of an animal from the egg-cell. This again is a most fitting process. The machine theory of life will consequently contend that the egg-cell contains devices which force the morphogenetic process to keep on the

[5] *Bethes Handbuch der normalen und pathologischen Physiologie, 15* (1931), p. 1077 ff.

fitting track. During its first phases the development consists mainly of cell-divisions. The egg divides into two cells, these again into two cells each, and so forth. Under normal circumstances each of the first pair of cells represents one prospective half of the future organism. It seems to follow that those devices which the undivided egg is supposed to contain must be distributed between its two daughter-cells; because here they would have to control what happens next in the further process. If the machine theory is right the same reasoning applies, of course, to subsequent cell-divisions. Driesch, however, was able to show that in the case of some species destruction of one cell of the first pair, again of one or several cells at later stages, did not prevent the development of typical organisms. From normally developed individuals they differed merely by their smaller size. Up to the time when the blastular stage of morphogenesis is completed, and followed by gastrulation, the experiment may with those particular species be performed in many different forms. This means in terms of machine theory that the loss of the most different devices does not prevent attainment of the normal product. When this fact has once been established, further argumentation will proceed along the same lines as it did in the last paragraph: For a moment one might assume that, besides those devices which govern normal morphogenesis, the egg contains regulating devices for cases of disturbance. It is, however, little likely that all the experimental disturbances which germs now undergo in laboratories have very often occurred in the history of the species. Thus evolution cannot have prepared the regulating devices whose existence the machine theory would have to postulate. It follows that there are no such regulators, although there is fitting regulation.

These arguments ought to suffice. Fitting organic function and regulation are doubtless possible under conditions to which the machine theory of life cannot be applied. Other factors besides anatomical devices must tend to make function fitting and, when circumstances require it, to replace normal processes by new modes of fitting function.

On Vitalism. We have so far followed a course which is supposed sooner or later to lead to Vitalism. Professor Driesch and his followers go indeed much farther than we have done in the last paragraphs. Since a machine theory seems to them the only interpretation of organic fitness which can be given in terms of customary scientific concepts, they feel entitled to conclude that causal harmony in the organism is beyond *any* explanation on known scientific grounds. Consequently they introduce a semimental agent that strives toward definite biological goals, directs organic events accordingly, and cleverly alters their course when this seems necessary in an emergency. This entity has several names; but usually it is called the Entelechy.

I will confess that I cannot move so fast. Granting that by Driesch's experiments the limitations of biological machine theories have clearly been demonstrated, I do not yet see why we should for this reason take such desperate measures. We should always remember: Not all physical systems are machines. Although every known system operates under some given conditions, a great variety of different functions are possible in many, and will be realized when the situation of such systems changes. In fact when constructing his machines man creates an altogether special type which has only few analogues among other systems

of inanimate nature. As a rule, I insist, these other systems have more than one or a few degrees of freedom; and those possibilities of function which they possess occur quite independently of any "regulative devices." Obviously Driesch is not justified in drawing his very general conclusion, until it has been definitely proved that no regulation toward a standard status occurs in *such* systems. There might be something in the basic properties of *dynamics* which makes for regulation where no devices do it.

This point did not escape Driesch's notice altogether. As a matter of fact he sometimes considered physical systems which are about as devoid of special constraints as possible. These systems are identical with those in the example which I used in order to introduce the concepts "degree of freedom" and "machine": A number of masses move under the influence of mutual forces, for instance of attraction; there are no constraints. Under these conditions, Driesch says, we observe no regulation, no tendency to approach a standard status. The fate of the system is different for each initial constellation, i.e., for all the various positions and velocities which the individual masses may have in the beginning.—This is indeed true for the particular dynamic case which Driesch discusses.[6] The next question would seem to be whether under no circumstances any regulatory tendencies are exhibited by the dynamics of a system. Driesch, however, is satisfied that his example excludes this possibility quite generally. He there-

[6] In a philosophical treatise I read recently that all systems will approach a standard status, that of equilibrium, when no disturbing conditions are present, and that such behavior simply follows from the principle of causality. This remark is not altogether correct, it is not applicable to the systems which Driesch here considers In the next section of this chapter the condition will be discussed which must be fulfilled in a system if the statement is to be true.

fore turns once more to the question whether by special devices that group of masses could be made to "regulate," i.e., to move from all possible initial distributions toward one standard configuration. This, he finds, is possible in principle; but even with a limited number of masses it would lead to an enormous complication of devices, since generally speaking a particular arrangement will be needed for each initial constellation from which the system is to reach the same standard status. Application of this argument to the case of morphogenesis or of any organic regulation makes him again conclude: Without or with constraining devices physical systems never have that power of regulation by which the organism is characterized.

Let us distinguish between regulation as such, i.e., a tendency of a system to reach a standard status from various initial constellations, and regulation in a more special sense in which the standard status has particularly valuable properties, as for instance in the organism. From the point of view of general theory, regulation even of the first type will deserve all our attention, because under appropriate conditions it might become regulation of the second type. Thus it must be emphasized that, notwithstanding Driesch's negation, regulation of the first type is an extremely common event in the physical world. I take a bottle and fill it partly with water. I shake it violently and then put it on the table. In a few moments the water is again quiet, its surface is a smooth horizontal plane. Whatever initial movement and distribution I may choose to give the liquid, it always returns to this final status. And yet, apart from the wall of the bottle, the water is not constrained by any special devices which force each particle to assume a particular position in relation to the others, and thus to contribute to such regulation. To this

extent the common final status toward which the water moves from countless initial constellations is reached for purely dynamic reasons.—Or consider a reversible chemical reaction in which the atoms A, B, C, D form molecules according to the formula: (A C) + (B D) = (A D) + (B C). If the four atoms be given in certain quantities, this reaction will always proceed until the molecules (A C), (A D), (B D) and (B C) are present in definite amounts; and these amounts are wholly independent of the proportion in which the same molecules are initially represented. Moreover, if in the beginning there is an overbalance in favor of (A D) and (B C), we find what actually happens by reading the formula from the right to the left; in the opposite case the reaction proceeds from the left to the right. And of course there are no devices which bring about such regulation.

Once more, it should not be argued that these instances are far too simple and trivial to offer any interest in an interpretation of biological regulation. Let us not confuse two different issues. We are now discussing the general question whether as a matter of principle the operations of any physical systems approach a definite final status independently of the situations in which their operations begin, and also independently of any constraining devices. We do not maintain that the very first examples of such regulation which occur to us are in any degree startling facts. Driesch himself tried to answer this question of principle in a simple case, and he was quite satisfied by the negative result which he found there. Thus any instances to the contrary, however trivial, are quite to the point in that they show that his survey missed an essential property of certain physical systems. Regulation as such *is* a characteristic of some systems or processes, in complete

absence of any such special devices as provide for regulation in certain machines. As to the more particular interest which our examples may fail to exhibit, there is no reason why such regulation should be confined to simple and uninteresting systems. At any rate Driesch's conclusion that only a semi-mental agent can produce regulation without devices is now invalidated.

A few years ago an important discovery was made which seems to militate directly against the Vitalistic doctrine. May I once more return to the early phases of morphogenesis. Even if a species be considered on which Driesch's experiments can be successfully performed, they do not succeed beyond a certain age of the germ. At the time when the blastula begins to be transformed into a gastrula, i.e., when a certain part of the blastula is invaginated into the interior of the embryo, this part is from now on found to have a fixed developmental future. Transplanted into any parts of a still "flexible" host germ, for instance, this particular material will develop just as it would have in its normal position. It is even more remarkable that, as H. Spemann and H. Mangold first found, the still flexible parts of the host adapt for the most part their own further development to that of the transplanted material.[7] Because of this property that part of the germ which first becomes developmentally inflexible during gastrulation is called an *organizer*. Under normal conditions developmental fixation spreads from the organizer over the whole germ. In other words, when the neighbors of the organizer assume a definite developmental rôle they do so for ever;

[7] H. Spemann and H. Mangold, *Roux' Archiv f Entwicklungsmech.* *100* (1924), pp 599 ff. *Cf* also here and for the following the excellent report "The so-called organizer and the problem of organization in amphibian development" which P. Weiss has given in *Physiological Reviews* *15* (1935), pp 639-674.

the same then happens to their neighbors, and so forth, until a definite program holds for all areas of the embryo. On the one hand this means that morphogenesis loses at this stage most of its astounding regulatory power; on the other hand all actual differentiation, the formation of all particular organs, begins in this period of the germ's life. Quite naturally therefore Vitalists seemed for some time to find the very essence of life located in the organizer which had proved responsible for such a decisive turn in the history of the individual.

This curious material, however, has quite recently been subjected to radical experimental tests by Holtfreter and other embryologists.[8] It was, for instance, killed by heating at temperatures in which no living tissue can survive. It was also boiled in water; or it was kept in alcohol for half a year, and so forth. When afterwards grafted into a host embryo the dead material proved as a rule to be still a good organizer. Even mere chemical extracts from the same material had this property. And yet one observation was still more surprising: Areas of germs which had not yet spontaneously reached the stage of organizers acquired the organizing faculty by such treatment as must necessarily transform them into inanimate material. These discoveries have been confirmed by many authors. They tend to prove that one of the most remarkable phases of morphogenesis is directly associated with a purely physical or chemical rather than with a specifically vital transformation. Some Vitalists seem now to assume that it is not actually the organizer, but the substratum, the still flexible new environment of an organizer, in which the very essence of organization is located when the organizer con-

[8] *Cf* J. Holtfreter, *Roux' Archiv f. Entwicklungsmech.* *128* (1933), pp. 584 ff.

sists of dead material; and that the organizer plays in such cases merely the rôle of a more or less indifferent stimulus. Even so, one would have to remark, it appears to be a most surprisingly effective stimulus. Moreover, some recent observations prove that the organizer must have more or less specific chemical properties if it is to be effective. In this field more discoveries are doubtless to be expected in the near future.—

Whatever the final interpretation of Holtfreter's experiments may be,—in its comparison of living systems with inanimate nature Vitalism has failed to investigate certain physical systems which actually "regulate" without any corresponding devices. It will therefore be our next task to examine such systems.

In discussing Vitalism Professor C. D. Broad distinguishes between the Theory of a Special Component and the Theory of Emergence.[9] Driesch's Vitalism is a special form of the first; Broad does not accept it. But he approves of the second, about which I should therefore like to make the following remark. Emergent Vitalism consists in the assumption that specifically vital states and events, although causally determined without any exception, follow *unique* and *irreducible* causal laws of their own. This seems to Broad an entirely satisfactory form of Vitalism. He explains it by pointing to chemistry where often the properties and the behavior of a compound cannot be predicted from a knowledge of its components or from that of any other compounds which the same components may form. That this has been the general situation in chemistry for many years did not prevent this science from progressing and extending quite rapidly. Similarly, Broad says, biology may study specific laws of still higher forms of organization, those of life, forever unable to reduce them to the laws of chemistry and physics, and yet progressing as a strictly causal discipline. This seems also to be the opinion of J. S. Haldane.

[9] C. D. Broad, *The Mind and its Place in Nature* 1925; pp. 60 ff.

In a way this theory gives a description of biological work as it appears at present, just as it describes what chemistry was some time ago. To precisely the same extent, however, chemistry was slower than physics in becoming a science in the proper sense of the word. Aggregates of unrelated and irreducible rules are not quite what we should like the sciences or science in general to be. To discover how under various circumstances *general* laws give rise to one or another particular class of events, this goal of research is for the scientist quite as important as is the collection of special rules, however strictly they may be followed in their narrower fields. Any sharp dividing line in human experience beyond which no insight can yet pass from one realm to the next is therefore a challenge to the scientist. His very best motives are negated if he is asked prematurely to accept such limits of understanding as Emergent Vitalism proposes. I admit, however, that the defenders of "Mechanism" have done very much to create such a Vitalistic resistance as is represented by Emergent Vitalism. From a general human point of view not a few will prefer to set life apart in a special reservation rather than to see it misrepresented as a super-mill.

III

In the discussion of Vitalism certain physical systems were mentioned which, although they are otherwise quite trivial, seem interesting in that they "regulate" in complete absence of any regulating devices. The essential characteristic of regulation is an invariance of *direction*. Whatever initial configuration may obtain in those systems when we begin to observe them,—if we observe long enough their inner displacements or transformations will always be found to bring them nearer to a standard status. The word "standard" points here to the fact that the final status is independent of the initial configuration.

If we ask a physicist whether any of his general prin-

ciples refer to the direction of physical transformations, he will tell us that there is one and merely one such principle: The Second Law of Thermodynamics. According to Boltzmann's theory, which seems to be generally accepted, this law is to be interpreted in the following way. When the macroscopic state of a system is given, the actual position and the behavior of its microscopic parts—molecules, electrons, and so forth—are not strictly determined by those data which serve to describe the macroscopic situation. In a gas, for instance, pressure is a macroscopic datum; and there are a great many combinations of positions and of velocities of molecules which yield as their macroscopic resultant the same amount of pressure. The number of microscopic constellations by which a given macroscopic situation may thus be realized is called the "probability" of this situation. To different macroscopic states of a system belong for the most part different degrees of probability in this sense. It has been shown that any macroscopic processes which occur in a system will *either* lead to states of greater probability, i.e., to states for which there are more chances of microscopic realization, *or*—but this is a rarer case—to states which have the same probability as had their predecessors. No processes will ever occur by which the system attains lower degrees of probability. This is in an abbreviated form the current interpretation of the principle that, according to the physicists, governs the direction of physical events: Systems change in the more probable direction. For reasons which will soon become apparent we must add that this principle applies exclusively to systems as closed wholes. It is not directly applicable to systems that absorb or emit energy in commerce with other parts of the world.

In modern physics such processes as diffusion, heat con-

duction and reversible chemical reactions are thought to be completely understood in terms of the Second Law. But to the same class belongs friction, the transformation of macroscopic displacements into irregular microscopic movement, or heat.

We can now see why from this point of view regulation must be a natural characteristic of many physical systems. Among all the macroscopic states of which such a system is capable there is one which can be realized by a maximum number of microscopic constellations. So long as the system is in a state that does not correspond to this maximum, further transformations are possible in the direction of higher probability. These will occur until eventually the maximum is actually reached. This will happen in whatever state the system may have been found in the beginning. For a given system the state of maximum probability is one; if therefore no special obstacles obstruct the path from some initial state to this unique status, the system will attain it under all circumstances. It is in this sense a standard status. And I need hardly add that this standard status will be reestablished if by some influence it should be temporarily disturbed.

An unsophisticated reader may not feel completely satisfield by this interpretation; he may find some mystery in this tendency of systems to transform themselves, as time goes on, in the direction of higher probabilities. We need not discuss such difficulties, however, because our next steps are entirely independent of the answer which either a physicist or the author might give the reader.

A standard status which is entirely due to this principle is called a thermodynamic equilibrium. Of the two examples which were mentioned in our discussion of Vitalism the standard status of a chemical system belongs to this

class. In physics it is customary to say that *all* equilibria are attained in the same manner. This would imply that *all* regulation in which we are at present interested—i.e., all regulation that is not brought about by regulating devices—follows from the Second Law of Thermodynamics. A moment of reflection will show us, however, that this is an ambiguous statement.

For this purpose let us consider our second example, the trivial case of water in a bottle. However much we may shake this fluid it will always gradually return to the same final condition. Is such regulation merely a matter of the Second Law? Under the influence of gravitation the water is pervaded by stresses which have various intensities and directions from one point of its interior to another, depending upon the distribution which the fluid happens to have. When in his description of the situation the physicist refers to the stress at a given point, he may use such terms as "force" or "pressure." He will then say that at each point there is a resultant gradient of pressure that tends to produce accelerations in its own direction. For the liquid as a whole, however, such a description would be of slight convenience, since both the direction and the intensity of these dynamic gradients will as a rule vary from one point to another. The same difficulty is met in many fields of physics. In order to overcome it the physicist introduces the concept "potential energy" which allows him to express by one mathematical function the totality of stress in a system, while from the same function he can always return to the local gradients by a very simple mathematical procedure.[10]

[10] The concept "potential energy" cannot be applied to all fields of physics. But those parts of physics in which it is not applicable have no significance for a comparison between the living system and physical nature.

At a given point, we said, acceleration occurs in the direction of the dynamic gradient which obtains at this point. When considering the totality of all accelerations throughout a system we have to speak in terms of potential energy, and then our rule will be: The totality of all accelerations, the effect of all gradients taken together, is for the system as a whole invariably in the direction of decreasing potential energy. This is a further principle that refers to the direction of physical events. It is entirely independent of the Second Law of Thermodynamics, because it holds even in those cases in which the probability of successive states of a system remains constant. On the other hand, this Law of Dynamic Direction does not as such determine what actually happens in a given physical situation. The behavior of systems to which this law applies depends on *two* factors: First, on the gradients and thus on this law; secondly, on the velocities with which the parts of the system move while the gradients operate. The gradients do not influence these velocities; they merely add to them new components. Consequently, if existing velocities are, for instance, opposite to and greater than these new components, the system may actually change in a direction which is opposite to that indicated by the Law of Dynamic Direction. And yet the law remains valid, namely for the totality of all effects which are produced by the gradients. The fact that the law holds merely for the accelerations in a system, not necessarily for its actual displacements has prevented the physicists from formulating it explicitly. I have no doubt, however, that it deserves the greatest attention. Without reference to it precisely the more interesting cases of equilibria cannot be fully understood.

Velocities, with which the parts of a system move inde-

pendently of the gradients that obtain at a given moment, are preserved by inertia. We may therefore call them "inert velocities." What would happen, if these inert velocities were somehow eliminated? In this case the direction of actual displacements would everywhere in the system coincide with the direction which the Law of Dynamic Direction indicates, and all transformations would for the system as a whole take the course toward lower amounts of potential energy. Suppose now that there is one constellation in which the system attains a minimum value of potential energy, and that no particular obstacles obstruct the path from any initial condition to this unique constellation. Under these circumstances the system will eventually reach this standard status in whatever configuration we may have found it at the outset; in other words, the system will now "regulate," because its displacements actually follow the Law of Dynamic Direction. In this case its standard status will of course be a configuration in which no further displacement is possible, because now all gradients balance each other. Thus it will be an equilibrium of dynamic factors.

We know of one main influence by which the inert velocities of a system can be eliminated. This influence is *friction*. Friction, we remember, transforms macroscopic velocities of physical entities into irregular velocities of microscopic particles, i.e., into heat. In many cases there is so much friction that any inert velocity is at once destroyed. Whenever this occurs macroscopic displacements in a system will of course lead directly to the standard status, the equilibrium. Under such circumstances there are at a given time only those velocities which the gradients at this time produce. Now, friction is one of the physical facts which belong entirely to the domain of the Second

Law of Thermodynamics. To this extent the Law of Dynamic Direction becomes a law of actual displacements merely when combined with the Second Law of Thermodynamics. It will nevertheless be obvious that the direction with which the former law deals is as such not given by the latter principle. That direction is implicit in the operation of *forces* whether or not actual displacements follow this direction. And the Second Law of Thermodynamics can do no more in this connection than abolish the inert macroscopic velocities. The direction which is then actually taken by the transformations of the system is a matter not of probabilities, but of the continuous operation of dynamic vectors.[11] If these gradients balance each other in one and in only one configuration, the system will, so I may now repeat, "regulate" toward such an equilibrium, precisely as other systems regulate toward a thermodynamic equilibrium. And again, just as the Second Law of Thermodynamics applies exclusively to systems as closed wholes, so the Law of Dynamic Direction holds for sufficiently isolated systems, not directly for any systems that absorb or emit energy. We shall soon have to discuss the full bearing of this limiting condition.

Here the reader may once more wonder, and observe that so much parallelism between two different principles—which both refer to the direction of physical events—appears to him slightly surprising. I will confess that I share his feelings. Once more, however, our further arguments will not depend on the attitude which this puzzling situation may evoke in us.

We can now clearly see why a liquid in a container returns to a standard status from any initial distribution

[11] For this reason no term that refers to the Second Law of Thermodynamics is mentioned in the expressions which define an equilibrium of actual forces. *Cf.* the author's article *Zum Problem der Regulation* in *Roux' Archiv f. Entwicklungsmech.* *112*, pp. 315 ff., 1927.

which it may have at the outset. The gradients by which the liquid is pervaded operate continually in the direction of the equilibrium. Inert velocities, however, which may exist in the beginning, or which originate during the following displacements, are gradually eliminated by friction. They become less and less relevant; more and more the Law of Dynamic Direction determines what actually happens, until eventually all movement disappears in the standard distribution of the fluid.—The example shows that with moderate degrees of friction the equilibrium may be attained only after some oscillations during which inert velocities carry the system repeatedly beyond its equilibrium. As the effects of friction accumulate, however, such oscillations will lose amplitude, and eventually they will altogether disappear. Such is the case in this particular example. May I insist that we know of many instances in which friction is strong enough to eliminate *at once* any inert velocities on the macroscopic level. Since in such systems the Law of Dynamic Direction holds without any limitation whatsoever, these systems will occupy us again in the following discussions.

As a second application of our theoretical analysis we may now consider Driesch's case of a dynamic system that does *not* "regulate": Masses move under the influence of mutual attraction; there is no regulation, inasmuch as any variation in the initial constellation leads to a particular course of events. We see at once why this must be so. Unfortunately Driesch's only example of a system without any special devices is at the same time a system without any friction; his masses move in empty space. Consequently the Law of Dynamic Direction does not in this case determine the actual displacements; it only determines accelerations. In order to make such a system "regu-

late" Driesch introduces machine-constraints, and he finds that regulation may be achieved if these constraints are sufficiently numerous and appropriately chosen. The same task, we know now, can be solved much more simply. Let all the masses move in a common viscous medium through which they pass with much friction, and the regulatory tendency will at once become apparent. From any initial configuration the masses will now converge toward the centre where they will gather in the densest possible group.

It is interesting to note that even thus regulation may not be complete. If the masses are of different sizes their distribution within one final cluster is likely to depend on their initial configuration. Why this deviation from complete regulation? It is caused by the fact that the system contains some elements of rigidity which cannot be influenced by the forces of the system itself. The volume and the shape of the individual masses are given as unalterable factors. In a similar way perfect regulation could be prevented by some rigid and fixed arrangement that would obstruct the path of some masses from their original positions toward the center. The Law of Dynamic Direction would still hold; but, in technical terms, the system would now have various "relative minima of potential energy," in each of which it could attain a balance of forces. Which of these it actually reaches would depend on the initial configuration.

In biology we observe certain limitations of regulation as well as positive facts of regulation. In comparing the living system with physical instances we shall, therefore, be greatly interested in such conditions as *restrict* the range of possible regulation. The most important cases seem to me in this connection such chemical reactions within the system as lead to a

precipitation of substances that cannot be dissolved by any agents contained in the system. Once such precipitations have occurred, they will not take part in further chemical regulation. Moreover, their presence will from now on constitute a self-created permanent constraint on the system; to this extent the state of equilibrium will never be again what it was before. One cannot help feeling that some such change may occur in an "organizer" at the time when it begins to deserve its name.

In fact, the rôle of rigid constraints is not generally confined to such disturbances of regulation. As a rule systems are subject to some unalterable conditions on which the standard status itself altogether depends. In our chemical example the atoms A, B, C, D are given in definite amounts; and these determine the chemical equilibrium of the system. In our other example the walls of the bottle do not act as regulating devices; but, apart from gravitation as a constant factor, it is principally their existence as rigid constraints and their shape on which the standard distribution of the fluid depends.

I need hardly emphasize the fact that instead of these two examples we might have chosen any number of others from various fields of science. No more instances will be needed in which regulation is directed toward a thermodynamic equilibrium. On the other hand, the Law of Dynamic Direction applies, for instance, in the following cases. Distort the shape of an elastic body as you wish, and, if the distortion has not gone too far, you will see the system returning to its "proper" shape from all those initial configurations. Give a conductor an electric charge, and, whatever its initial distribution, this charge will distribute itself at once in one specific pattern that depends on the (outer) shape of the conductor and on the properties

of its neighborhood, but not on the initial pattern.[12] When electric currents distribute themselves in a given medium they follow the same law.

If several kinds of processes, states and materials are combined in one system, regulation will still follow from the same principles: the Second Law and the Law of Dynamic Direction. Thus a system may have hydrostatic and electrostatic characteristics, diffusion and chemical reactions may occur in it, and so forth; again, one of its parts may be conspicuous by a particular form of events, a second by others. Instead of one form of potential energy, it is true, we have in this case to consider a more complicated expression which comprises all varieties of available energy contained in the system. But for this complex indicator the same rules hold as have here been given for the case of simpler and more homogeneous systems. Since, however, regulation in heterogeneous systems is generally associated with the transformation of certain forms of energy into others; since, furthermore, one part may operate in one way, while in another processes may belong to a different field of physics, and so on,—such regulation will now produce a curious impression. The various processes will seem to have agreed to coöperate toward the standard state of the system as a whole, as though it were their common goal.

The reader will long since have noticed that with our present discussion we have returned to problems which we considered once before when dealing with instances of "macroscopic self-distribution" (*Cf.* ch. 6). The last paragraphs contain indeed an abbreviated and elementary

[12] In the case of electric phenomena self-induction plays a similar rôle as does inertia in mechanics. The effects of self-induction, too, are eliminated by friction.

theory of such self-distribution, an analysis which will be completed in the last section of this chapter. With the same theory Mach's principle is of course directly connected. This principle, we remember, implies the existence of "unique" or "outstanding" patterns of distribution (*Cf.* ch. 7). Such particular distributions are obviously those in which given conditions allow the gradients or vectors of a system to balance each other much more thoroughly than they can do in other cases.

Besides the Second Law and the Law of Dynamic Direction a third principle is sometimes mentioned which also refers to the direction of physical events: the Principle of Le Chatelier. It does not tell us what a physical system will do when its general conditions, its constraints and so forth, are given, but rather what its reactions are when these conditions themselves are changed. We may formulate the principle in this statement: If one of the conditions be altered under which a system is in equilibrium, some internal factors will change in such a manner that the new equilibrium differs as little as possible from the previous standard. The system seems to defend its own existence. I do not intend to discuss this rule, since we do not yet know whether it really deserves the name of a principle. As yet nobody has been able to find a completely satisfactory enunciation of it, not even Planck, who recently gave it much attention.[13] For biology it is undoubtedly a serious desideratum that the principle be soon clearly established; or else, if there is no such general law, we ought at least to learn why and under what circumstances systems behave as though they followed such a rule.

IV

As it became more and more apparent that the machine principle is not capable of giving us a satisfactory

[13] M. Planck, *Annalen der Physik*, 5. Folge, *19* (1934).

explanation of organic regulation, an interpretation in more functional or dynamic terms began to attract some theorists. At first it seems indeed a plausible assumption that in the organism fitting regulation toward a standard status occurs for the same reasons that make physical systems attain or reëstablish an equilibrium. Unfortunately, however, the concept of "equilibrium" is in this connection often used in just as vague a meaning as had previously been the case with the concept "machine." It appeared therefore advisable to analyze physical regulation before a comparison was undertaken between the normal state of an organism and an equilibrium in physics.

On the face of it, these standard states seem to resemble each other in a most promising manner. There is besides a special point which gives an equilibrium theory of organic regulation a particularly inviting appearance. Physical systems, we have found, tend to transform themselves in the direction of an equilibrium for two reasons: either because their processes follow the Second Law of Thermodynamics, or because the Law of Dynamic Direction applies to them. Now, even the most superficial consideration of the organism must convince everybody that its normal state cannot be a mere thermodynamic equilibrium. If, therefore, an equilibrium theory of organic regulation is to be at all proposed, this can be done only with the premise that both the Law of Dynamic Direction and the Second Law apply to the organism; in other words, that the organism regulates toward a balance of directed vectors no less than it does toward "a most probable situation." We have seen, however, that the Law of Dynamic Direction does not determine what actually happens in a system, unless there is sufficient friction by which inert macroscopic velocities are eliminated. Is this condition

fulfilled in an organism? Without any doubt it is. In the movements of our limbs and in circulation inert velocities may perhaps play a modest rôle. In the tissue, however, friction is as great as it is in the interior of any solution. Consequently there are no such velocities in the tissue. What happens here must, from the point of view of physics, follow either from the Second Law or from the Law of Dynamic Direction. In the former case we could say with the physicists that changes will occur in the direction of "higher probabilities"; in the second case, that displacements will be proportional to, and in the direction of, the vectors which happen to obtain at each point.

The fact that the organism contains many "devices," i.e., relatively permanent conditions of function, implies no obstacle for an equilibrium theory of organic regulation. In a physical system there may be many constraints; and yet, within the limitations which are thus imposed upon its operations, the tendency toward an equilibrium will determine what actually happens. We have only to realize that the equilibrium in question will itself respect those limiting constraints. In this sense the principal idea of all machine theories is entirely compatible with the more dynamic or functional notions to which an equilibrium theory refers.

On the other hand, it is the existence of relatively rigid anatomical conditions which restricts the range of possible organic regulations. Such devices as the organism possesses are undoubtedly apt to give the tendency toward standard states a general direction which is particularly fitting under more or less normal conditions. At the same time they exclude, precisely among the higher veterbrates, some regulations which might otherwise occur even in quite *un*usual situations. Such anatomical facts

are, as it were, "not made for these conditions"; and since they do not yield to the stress of altered function they prevent, under these circumstances, the actual occurrence of complete regulation.

So far, then, an equilibrium theory of regulation may seem to be wholly compatible with what we know about the organism. Any interpretation of organic fitness that does not take account of our two functional principles appears to me indeed as fundamentally unsound. On the other hand, it is equally true that neither the Second Law nor the Law of Dynamic Direction can be applied to the organism in that simplest formulation in which they refer to equilibria. I have been at some pains to make the meaning of these principles more explicit than is often done, because I wish it to be perfectly clear that, unless a much broader view be taken, an equilibrium theory of organic regulation would be entirely misleading. To express the main argument against such a theory quite briefly: Neither is the standard state of an organism a state of equilibrium in the common sense of the word, nor do organic processes in their totality generally tend to approach such an equilibrium.

In the introductory chapter of his book Cannon remarks that "the constant conditions which are maintained in the body might be termed *equilibria*." The author does not say what relation he assumes to obtain between this functional principle and his own view according to which regulation seems always to be due to regulating devices. At any rate, he prefers to give the name *homeostasis* to the fact that certain "steady states" are so obstinately preserved or reëstablished in the organism. Equilibria, he adds, are found in simple closed systems, "where known forces are balanced." Again, he says, the word homeostasis "does not

imply something set and immobile, a stagnation." [14] There is something in these last words which many biologists may appreciate when attempts are made to explain organic regulation by an "equilibrium theory."

Convincing objections may, in fact, be raised against any such attempts. First, as Cannon says, no organism is detached from the rest of the world to an extent that would make our principles directly applicable to living systems. These systems are not closed. They absorb and they emit energy. At times they absorb much more than they emit. From the point of view of physics it is, therefore, simply impossible to state it as a rule that transformations in organisms occur in the direction of equilibria.

The same follows from the fact that in a healthy normal condition many vertebrates are by no means in equilibrium with regard to their immediate environment. Mammals, for instance, *stand* when at rest; for the most part they lie down only when slightly or seriously fatigued. Many fishes assume when at rest a position in which their heavier parts are turned away from the direction of gravitation. And yet in a state of physical equilibrium the center of gravity both of mammals and of fishes should be lowered as far as possible. Since no outer physical forces keep the mammals standing and the fishes swimming against the pull of gravitation, i.e., in an unstable position, such organisms must, when fresh, healthy, and therefore in their standard state, contain sets of vectors and processes which *prevent* the attainment of an equilibrium. These factors represent a certain amount of potential energy. But no physical system that is as such in a state of equilibrium can at the same time preserve an energy reserve by which it avoids reaching an equilibrium with regard to the en-

[14] *Ibid*, p. 24

vironment. In the present example those factors seem even to keep the organism in a state *that departs from an equilibrium as much as possible.*

No conclusion other than this can be drawn from what happens during the development of individual organisms. During youth the standard state, for instance of man, varies slowly with time. There is always regulation toward a state that may be called temporarily normal. But from month to month, and from year to year, this state shifts gradually; and it is obvious that regulation changes its "goal" correspondingly. Thus, if we know in what direction the standard state of the full-grown healthy adult differs from that of the healthy child, we shall also be enabled to define in what direction, toward what kind of state, regulation occurs when the individual is fully developed.—We see the answer at once. In a state of equilibrium, as defined in this chapter, a system contains the smallest amount of potential energy which is compatible with given conditions. Potential energy, however, is the capacity of a system for macroscopic activities. Nobody, I am sure, will contend that a man of thirty can do less macroscopic work than can the new-born child. Just the contrary is true. From the point of view of physics the adult contains tremendous stores of potential energy when compared with the child. It follows that, in the healthy individual, development toward adult life is associated with an *increase* of such energy, that accordingly during this period regulation occurs in the direction of ever higher levels, and that, when development is at its peak, it is a *maximum,* not a minimum, of available energy which regulation tends to preserve.

These arguments must, I believe, convince everybody that an "equilibrium theory" of organic homeostasis is

not compatible with elementary biological facts. What is the theorist to do in this situation? In Professor A. V. Hill's words: *"If there be no equilibrium, how far dare we apply rules and formulae derived from the idea of equilibrium?"* [15] In several statements the same author hints at a possible answer. All physiologists, he says, "must have exercised their minds as to the reason why a living cell, completely at rest, and doing nothing at all except maintain its continued existence, requires a continual supply of energy." [16] For instance, "apart from any motor activity at all, a human muscle cell . . . uses, to maintain itself alone, about 30 calories of energy per gramme per day." [17] We are, he finds, thus forced to adopt "the conception of a dynamic steady state maintained by a continual expenditure of energy." [18]

I should like to add that there is nothing hazardous in this conception. We can easily give it a clear functional meaning if we consider one more physical example. Life has sometimes been compared to a flame.[19] This is more than a poetical metaphor, since, from the point of view of function and energetics, life and a flame have actually much in common. The flame, say, of a candle is a steady state. The continued existence of this state involves a continual supply of potential energy which the flame receives as "food" through the wick and as oxygen from the air.[20] When undisturbed, the flame remains the same in size and in shape. Thus one might be tempted to believe that its status is that of an equilibrium. But in the sense

[15] A. V. Hill, *Adventures in Biophysics* (1931), p. 60.
[16] *Ibid.*, p. 4.
[17] *Ibid.*, p. 60.
[18] *Ibid.*, p. 62.
[19] *Cf.*, for instance, W. Roux, *Die Selbstregulation* (1914), p. 17, pp. 79 f.
[20] Chemical energy is often treated in such cases as a form of potential energy. In fact, in all respects which concern us here it *is* potential energy.

in which this word is commonly used the flame is certainly not in a state of equilibrium. In order to see this we need only apply the same test to which we just subjected the adult organism: What is the genesis of a flame? We light a candle with a match. On the wick there appears at first a tiny flame. This flame grows spontaneously until it attains a maximum size and at the same time a certain shape, which then remain unaltered. If during the initial phase we hold our hand near the flame we can easily feel that quickly *increasing* amounts of heat are emitted at this time. We also see that during this period the flame throws more and more light on its environment. Any energy, however, which the flame emits at a given moment was just before this moment inner energy of the flame itself; again, a moment before, it was potential (chemical) energy that was ready to be transformed into heat and light. From our simple observation it follows, therefore, that during its "youth" the flame attains ever higher degrees of potential energy, and that in its final stationary state it contains a maximum of such energy. In this sense the steady state of the flame departs as widely from a condition of equilibrium as it possibly can.

The factors which determine the maximum energy of the flame may be indicated in a few words. As soon as the candle is lighted, "food" which is contained in the wick and oxygen that is contained in the air are being spent by combustion. Thus gradients are set up both for the food and for the oxygen. The flame begins to grow, and these gradients increase correspondingly. Higher amounts are therefore supplied both of food and of oxygen. But there is a limit to this process. When a certain size and a certain maximum of combustion have been reached, any further growth of the flame would lead to a higher demand than is compatible with the possible speed of oxygen diffusion from the surrounding

air and with that of the food-stream which passes through the wick.

We are now in a position to apply our theoretical concepts to the flame and then to the organism. The flame is not a closed system. It can, however, be considered as part of a larger system for which our general principles are valid. If this be done certain consequences will follow for the behavior of the flame as such. The air of the environment and the substance of the candle, taken together, contain amounts of chemical energy which are to all practical purposes unlimited. If therefore the material of the candle and a sufficiently large volume of air are included, we obtain a "system" which we may regard as closed; because during the lifetime of the flame the energy on which its steady state depends will be exclusively supplied by the candle and this volume of air. An untrained observer's attention may be completely concentrated on the flame as an outstanding visual fact. From a functional point of view, however, the life of the flame can be understood only in the context of that larger "system." This system follows our general laws. The changes which occur in it must as a whole have a direction which lowers the amount of potential energy contained in the system. So long as the candle is not lighted this chemical energy cannot be spent at all. On the other hand, once a sufficiently high temperature is created at the tip of the wick, energy begins to be spent by combustion. And the more the flame grows, the more energy will be expended per unit of time.

It comes then to this: Our system consists of, first, a practically unlimited store of chemical energy which, however, cannot be directly spent; and, secondly, of a minor part, the flame, in which this energy *can* be spent up to a certain maximum rate. The "system" as a whole will lose

its potential energy the more quickly, the more of this energy streams into the only part of it in which it can be expended. This is the flame. For this reason a maximum of such energy migrates steadily into the flame; for the same reason the flame *contains* continually a maximum of potential energy. Only thus is it enabled to expend energy at a maximum rate.

The fact that the stationary state of the minor system involves a maximum of potential energy is entirely compatible with our general principles. These refer to closed systems; and in the closed system of the present example, taken as a whole, events have precisely the direction which is prescribed by those principles. On the other hand, it is obviously an essential observation that in the only "working" part of this closed system the direction of events is just the opposite of that to which the principles refer. The activity of the flame as such, it is true, namely combustion, tends to lower the amount of potential energy in the flame. But this does not really happen because any spent energy is at once replaced by a corresponding new supply. The more energy the flame emits, the higher is the rate at which it is supplied. Thus the flame is continuously fed with the greatest possible amount of energy.

A general view of the organism shows us a situation which resembles strongly that of the flame. The organism is not a closed system; it is part of a larger functional context, the external section of which contains as its most important components oxygen and food, i.e., a store of chemical energy which may be regarded as practically unlimited. In one respect there is a difference between the flame and the organism: Unlike the flame, the organism itself normally contains great reserves of food in the widest sense of the word; it is stored, for instance, in the liver.

From these sources rather than from the outside other tissues receive their food supply directly.

The potential energy of oxygen and food is not spontaneously spent outside the organism; nor is the food reserve consumed where it is stored within the organism. All "activities," however, of which the organism is capable do tend to lower the supply of chemical energy that is contained in the active tissues. This is in line with our principles. But it is also in line with these principles that under such circumstances the stores of food deliver new supplies, and that these tend to maintain or to reestablish the highest energetic level of the tissues. If we compare this situation with that obtaining in a flame, we shall expect the active organism to heighten its content of potential energy during youth, and to preserve this content when a maximum is reached. This is exactly the behavior of the organism to which I pointed when I showed that the standard state of the organism cannot be a state of equilibrium.

Suppose now that at a given time there exists in the organism only one state of the tissue which corresponds to a maximum of potential energy. If by considerable work or by any other influence an organ or some larger part is changed so that for the organism as a whole the maximum condition is no longer maintained, such processes will occur as will bring it back to higher levels of energy. And since there is only one standard state in which a further increase is not possible, the organism will from various initial states "regulate" toward that maximum condition. In other words, so far as regulation is concerned, our previous discussion of standard states applies to a maximum condition, just as it did to an equilibrium. Of course, it applies quite generally. There will, for in-

stance, be in this case the same influence of given anatomical constraints as was mentioned in the case of regulation toward an equilibrium. Again, regulation toward a highest standard state will only be possible within limits which are given by relatively unalterable anatomical facts.

Actual regulation, however, will now be characterized by one remarkable trait. On the highest possible level of potential energy a system is capable of doing more macroscopic work than it is on any other level. If, therefore, in the organism any change by which this level is lowered tends to be followed by processes which counteract that change, and thus reëstablish the highest level, regulation will serve to keep the living system in its most powerful state and, in this sense, to protect it. This, it seems, is the condition in which the various tissues are maintained by a constant supply of energy, and which is so often spontaneously restored after disturbances.[21]

I realize that in the last paragraphs no more than a general outline has been given, which cannot become a theory until a great many biological facts have been considered from this point of view. Since this task does not belong to our present program I shall only add a few tentative remarks.

One might ask why with these premises an organism does not live forever. My answer would be that regulation has its limits for reasons which I mentioned before;

[21] I have sometimes been asked why I refuse to call the standard state of the organism "an equilibrium." My reason is simply that this standard state is not an equilibrium in any sense which has as yet been defined by science—not even an *unstable* equilibrium. It is a *stationary process;* and we are just beginning to learn that there are two classes of stationary processes, one with which a minimum, and another with which a maximum of energy is associated. Nothing could be more unfortunate than an attempt to hide such new essential distinctions behind an outworn general term.

and that, therefore, a great many influences are able to destroy life. It is quite as obvious that without a sufficient supply of food and of oxygen the level of life will soon sink. For the same reason organisms can die from exhaustion. But they seem also to deteriorate spontaneously when a critical age has been passed. It appears to me quite likely that practically all our activities tend slightly to alter the tissue in a way which does not at once disturb further function, but which cannot be fully compensated for by the metabolism and its regulatory tendency. If such changes accumulate for years they may gradually make the tissue or certain organs less fit to respire and to absorb food. What this would mean is fairly obvious.

A further question refers to the manner in which the organism obtains its food. So long as within the organism there are reserves of carbo-hydrates, fats, and so forth, the situation seems simple enough. But these reserves are limited, and I want to make it quite clear that the way in which the organism replenishes its stores is something altogether different from the simple processes that feed a flame with chemical energy. Delivery of both food and oxygen is in the case of the flame entirely automatic and direct. The existence of the flame sets up gradients in both respects, and those materials move in the direction of the gradients. Not much oxygen penetrates in this simple manner into the interior of a bird or a mammal; nor does food migrate into our interior simply because we are spending energy there. In the case of food, for instance, what happens is more complicated: Among our activities there is one group, that of finding and of eating food, in which we *expend* certain amounts of energy, just as we do in all the others. As a result of these activities, however, we *absorb* under favorable conditions much

more potential energy than is spent in order to obtain it. Thus the organism stores new supplies which enable it to maintain its raised normal level and, besides, to be active in other directions. In this there is nothing that could unbalance the energy budget; there is no contradiction between such operations and our general principles. None the less it must be noted that this particular behavior cannot be *predicted* from such principles, and that, from a general functional standpoint, it is a remarkable trait of living systems, about as remarkable as their reproductive activities. An actual theory of the organism will, if I am not mistaken, meet its most fascinating problems here.

There is no apparent reason, however, why science should hesitate to deal with the problem of *regulation.* Nor is there any essential difficulty in the fact that organic regulation is generally directed toward an "optimum" state. The physicists, it is true, have not given much attention to this functional possibility, although it follows from their general laws. Also, a thorough investigation of *heterogeneous* physical systems and of their regulatory behavior would contribute greatly to a further understanding of organic fitness. But even now we can predict from known principles that some such systems will show an impressive causal harmony by which they keep themselves in a "healthy" condition, as though this were their goal.

It is perhaps too early for final statements in this field, particularly since we know so little about the way in which evolution has created the living world. As this world now is, however, the following seems to me a conservative description of the situation: No procedure of science reveals any actual participation of demands and

values in the determination of organic events. At the same time, science can clearly demonstrate that in certain systems function will, for dynamic reasons, take a most "fitting" course. We do not discover requiredness as such among the data of science. But a general trend of nature is sometimes found to yield the same results as might be expected if the events in question were actually happening in order to fulfill a demand.

CHAPTER IX

FACTS AND FORCES

I

OUR analysis of regulation has given no support to the idea that requiredness plays a part in the determination of organic events. It seems possible to regard the continuance of life as a value that resides merely in the psychological realm. It might be said that we are interested in the prolongation of our own lives and in that of the lives of certain other people, but that actual regulation which "protects" these lives occurs for other reasons. In fact, the principles on which such regulation depends do not appear to be different from those by which the fate of inanimate systems is determined. Thus, if requiredness does not belong to the characteristics of the *physical* world, we need not attribute it to the *organic* realm.

It might be argued that an assumption which we are not forced to make may nevertheless have a high degree of plausibility. But what would follow from this particular assumption? If no essential problems have been overlooked in our analysis of regulation, the general theories of physics seem to *suffice* for an explanation of fitting organic function. With this premise no place of its own is left for requiredness as a determinant of organic processes. The assumption that requiredness plays a rôle in the operations of living systems would thus lead to a curious

inference: Requiredness would have to be identified with some phase of physical dynamics. Few will be inclined to accept this conclusion unless it be supported by very strong arguments.

Perhaps no positive results could be expected from an analysis of purely biological facts, since requiredness does not occur among the actual observables of this field. It seems therefore advisable to return to those instances in which requiredness is directly felt to extend beyond the phenomenal field and in this sense to be "amphibian." I do not hesitate at this point to summarize once more our previous discussion of transcending requiredness, since this concept will prove to be essential for the argument that follows.

At the end of the seventh chapter we found, for instance, that we are not quite free to complete a temporally extended context arbitrarily. When such a context is near completion its earlier parts often ask for a certain closure, and they object to others. The former appear as right, while the latter are felt to be wrong. This observation involves more than a confirmation of the fact that requiredness is a characteristic of contexts or structures. Not infrequently the earlier parts of the context in question have disappeared from actual experience at the time when one completion is accepted or another rejected. And yet both acceptance and rejection may altogether depend on those earlier parts. Often it is principally *their* properties which determine what at the present moment appears as right or as wrong. Moreover, the demanding vector that accepts one completion and rejects another is directly felt to issue from those parts and to owe its specific nature to their characteristics. These characteristics as such are no

longer phenomenally given. But in its reference to the immediate past the vector implies both that there are those hidden parts, and that its present demand is due to their equally hidden properties. To this extent it is possible for requiredness to emerge from beyond the phenomenal world and to exert its selecting influence within this world. On merely phenomenological grounds, of course, we cannot decide what kind of entity the beginning of a context is when it has ceased to exist as an experience. In terms of biological theory, however, an answer may be given. From this point of view the entity beyond can only be the trace of those processes which accompanied the earlier parts of the context when they were experienced.

For various reasons a second example appeared as even more conclusive. When trying to recall some previous experience we cannot always achieve its actual reproduction at once. At the same time we may find ourselves referred to an entity beyond the phenomenal field which, even in its hidden location, is felt to be "the right thing." This means of course that the entity beyond already fulfills certain demands which issue from a *given* context; in other words, that its transphenomenal location does not prevent it from satisfying a particular requiredness.—On the other hand, a demanding vector may also *issue* from the entity beyond. Since we know that this entity is "the right thing" we often confront it in a tentative way with such phenomenal data as may possibly approximate its actual nature, and from beyond come the answers: "quite wrong," "better," and so forth. Thus those data may be accepted or rejected with reference to an entity the specific properties of which are not phenomenally given. Again,

a merely phenomenological analysis can go no further. Once more, however, from the standpoint of biological theory this transphenomenal entity must be a memory trace.

It requires a certain amount of courage to conclude that parts of the past whose existence is now in the realm of traces may fulfill an experienced demand; that in this sense they may be *right*. It is at first an equally disturbing thought that a demand may issue from a trace, emerge into the phenomenal field and there decide what is wrong and what is right with reference to that trace. And yet I cannot see how these conclusions can be avoided.

I admit of course that in our second example, that of imminent recall, an *experienced* interest plays a certain rôle. We do want to recall and to recall correctly. But the presence of this wish does not as such explain our phenomenological observations. Before actual recall there is no *phenomenal* datum which could appear as right or as wrong, however much we may wish to remember. On the other hand, the entity beyond does so appear because of properties which are as yet completely hidden. Their fitting nature makes this entity "the right thing" with regard to our particular demand, independently of any phenomenal representation.—Again, when in a case of delayed recall we apply the tentative procedure and consider one possibility after another, there is of course a subjective interest at work. But whatever other influences this interest may have it cannot possibly decide whether one proposal is better than another. What should be the standard of comparison? A decision can be made and is actually felt to be made only by the standard beyond, with reference to which one proposal fits and another does not.—In those other examples,

moreover, in which the completion of a temporally extended context occurs under the influence of its beginning, the rôle of subjective interest may become altogether negligible. I hear somebody playing the piano in another room. He seems to give up in the middle of the melody. But when I have just turned to other matters he unexpectedly strikes the next note. This may at once be heard as wrong or right; and the protest or the acceptance comes from the recent past, the incomplete melody, quite independently of any subjective interest. If I *become* interested it is because the note is right or wrong in its relation to this hidden past.

C. D. Broad has given an analysis of memory-situations with many parts of which I find myself in full agreement. In several of his statements it is clearly implied that we may have commerce with a past object even though this object is not actually recalled. He also remarks that a certain given or proposed datum may be felt to fit or not to fit such a past object. At the same time, he adds, this object "is presented only to thought as the subject of such and such propositions." [1] This statement, I believe, is a trifle too non-committal. After all, the hidden object is felt to be such that it can decide about the fitting or the non-fitting character of proposed data. To this extent it has obviously quite specific properties. And since such specific characteristics are not experienced as phenomenally given, but as hidden somewhere beyond the phenomenal field, we are led to our statements about transcendence and about transcending requiredness.

May I now return to the example of imminent recall and see what else we may conclude from it. In this instance, we found, it is possible to examine a trace by confronting it with test-objects. For this purpose we choose

[1] C. D. Broad, *The Mind and its Place in Nature* (1925), p. 248.

such objects as are supposed to be more or less like "the right thing beyond." Our procedure is equivalent to so many questions; and the trace beyond gives as a rule quite definite answers which refer to each test-object in turn.

A trace is one thing, and a test-object, i.e., a proposed datum with its cortical correlate, is another. When accepting or rejecting this datum the trace obviously "does something" about it. How can a thing be represented "beyond itself" and thus do something about a second thing? Since the influence in question issues from the trace and is determined by its properties, we seem justified in assuming that at its point of origin this influence is a physical fact. I know of only one class of physical facts that represents the properties of a given entity beyond this entity and thus can "do something" about a second thing with reference to the first. This is the class of "forces" or "fields."

We found, however, that imminent recall may be considered from still another point of view. The entity beyond is felt to fit a given context and thus to be "the right thing." This entity we take to be a trace. It fits the given context by its specific properties. But these properties as such are not experienced; they belong to the physical realm. In the present case, then, an influence extends from the given phenomenal context to the trace in question and accepts it as right. Thus the context "does something" about the trace in reference to the characteristics of this trace, which are *physical* characteristics. It is difficult to imagine how this could happen, if at the locus of its object and effect the influence itself were not a physical fact. And again, I know of only one class of physical facts that issue somewhere and then exert elsewhere specific influences on physical objects according to the

properties of these objects. It is the class of "forces" or "fields." [2]

Although we have twice come to the same conclusion, this conclusion may not appeal to many because it does not lie in a direction in which they would like to proceed. On the one hand, it might be said that Positivistic criticism has long since demolished the concept of force; and that, if the term is still used in physics, it is used merely in a mathematical sense, as a convenient auxiliary construct without any immediate physical significance. In the last paragraphs, however, the words "force" and "field" seem to be given such a significance.—On the other hand, one might argue that I have been diluting the meaning of requiredness in an altogether inadmissible way. From the phenomenological point of view requiredness involves *acceptance* and *rejection* of one thing by another or by a context of others. The thing which is accepted or rejected *fits* or *fails to fit* given conditions. This description holds whether requiredness is fully surveyable within the phenomenal field, or whether it transcends this field. In view of these essential characteristics, one might say, we are hardly justified in interpreting requiredness as though it merely meant that one thing "does something" about another thing.

I am not convinced that the first argument is very strong; but I admit the force of the second. It is true that in all cases of requiredness "something does something

[2] The term "field" has two different meanings. In one sense it refers to such entities as "areas" Thus we speak, for instance, of the field of vision or of the phenomenal field—In a second sense the same word was first used by the physicists. It then refers to dynamic vectors that extend from an object into its environment. In a similar meaning it also refers to the specific distribution of such vectors in given cases It is obvious that in the present connection the term has this second, the dynamical, meaning.

about something else." But this statement does not seem to give a sufficiently specific expression of what requiredness means; it might also hold of other forms of reference besides requiredness. What we need at this point is obviously a more adequate analysis of this concept. Such an analysis has been attempted in the third chapter (pp. 72 ff.), but in terms which still referred to subjective valuation rather than to requiredness in general. It may therefore be advisable to emphasize once more the essential characteristics of situations in which this factor is involved.

First: A datum, an entity or an act is required *within a context* of other data, entities or acts. This holds both for negative and for positive requiredness.

Since the structural nature of all requiredness is implied in this word, but not in the term "value," I have generally avoided the second term although it sounds better as a word.—I have, moreover, given little attention to the fact that such value-predicates as "right" or "good" or "wrong" are often used with reference not to parts of contexts, but to the contexts themselves. I regard this as a minor issue provided that the structural nature of requiredness be recognized not only in the first but also in the second case. A context as such is acceptable inasmuch as its parts fit each other; it is wrong if one of its parts is not a fitting member of just this context.—Of course, contexts as such may also be objects of positive or negative 'subjective' valuation and thus become members of a larger context (*Cf.* ch. 3, pp. 98 ff.).

Secondly: Within the context in question requiredness is a *dependent* characteristic that has no existence of its own, apart from the entities that fit or do not fit each other in these contexts.

Thirdly: All requiredness *transcends* from certain parts of a context to others of the same context. Like all other kinds of reference, it is in this sense a directed translocal

trait, a vector, that cannot be split into bits which have a merely local existence.

The term "transcendence" does not at this point refer to a transition from the phenomenal realm into another, or vice versa. Where such a transition actually occurs we have to deal (a) with transcendence in the general present sense, and (b) with transcendence in that more particular meaning which is exemplified by our instances in the field of memory. *All* references "transcend" in our present sense, i.e., they are essentially not "things," but rather "steps" from one thing to another. To this extent they "transcend" even when both the things in question and the references themselves are clearly given in the phenomenal field.[3]

Fourthly: Requiredness differs strikingly from other forms of reference by its *demanding* character. It involves acceptance or rejection of the present status of the context in question, often more particularly, acceptance or rejection of some part by the remainder of the context.—This demanding character has degrees of intensity. The lower this intensity, the more will a condition of merely factual relation, juxtaposition or sequence be realized.

The adequate phenomenological analysis of requiredness appears to me as an extraordinarily difficult task. As soon as we try to go beyond the simple facts which have here been enumerated the complete lack of sufficient preparatory work in this field becomes most disturbingly apparent. I shall, however, mention two further properties which requiredness often exhibits.

Mere acceptance or rejection do not always exhaust its possibilities. A part is often not simply either wrong or right in its context. If, for instance, a context is completed

[3] It is perhaps advisable to add that the words "translocal" and "local" need not have a strictly spatial meaning. In many cases they would, for instance, have to be understood in a temporal sense.

in a way that approximates, but does not actually reach, a perfectly right condition, requiredness tends to assume a perfectionist or correctionist character, inasmuch as the direction in which the given situation differs from a perfect condition may be directly indicated in the given situation. Thus, if I am hungry, food is welcomed in my environment even when distant; it is to this extent accepted. But there is this additional trait in the situation that the food "ought to be nearer." Similarly, if a visual configuration is not entirely balanced a certain part may appear not simply as "out of place," but as "a trifle too high," "too much to the left," "too heavy," and so on. In this sense requiredness often tends to *improve* given situations by pointing to changes which would result in such improvement.

It seems to me that such a positively correctionist behavior of requiredness will be found merely in cases in which a continuous transition is possible from the original situation to the more perfect condition.—On the other hand, even negative requiredness may be considered from the same point of view. Rejection as such points to *removal* of a wrong part as a way of achieving a better condition. Thus negative requiredness, too, has as a rule a perfectionist character.

I should like to mention another common, but not quite general trait of requiredness. Acceptance or rejection may be clearly based on definite characteristics of those facts which are found to fit or not to fit each other in given contexts. In logic and also in aesthetics we have many cases in which there is to this extent *insight* into the actual foundations of requiredness. We cannot contend, however, that all requiredness follows the same rule. Requiredness as such may be most intense, and yet our insight into its foundation may not go very far. Certain

colors, for instance, do not fit each other. Sensitive persons avoid these combinations. But if we were asked by what properties precisely these colors become incompatible we should probably not know an answer. The same is true in many cases of 'subjective' valuation. Here one part of the context in question is the self, and something in the environment is the other part to which our positive or negative attitude refers. We may often find it easy to indicate approximately on what properties of an object our liking or disliking is founded. But we do not realize with the same clearness on what characteristics of our own self these attitudes depend. For this reason it is, and often remains, so difficult to understand certain valuations of others, for example in the field of art or in that of sex.

May I remind the reader that here, just as in the third chapter, phenomenological description does not merely refer to such particular values as have places of honor in books on ethics, aesthetics and logic. Our topic is requiredness in the generic sense, i.e., all facts without exception in which beyond mere existence and occurrence there is an "ought" or an "ought not." Thus the urge toward revenge after an offence may become very strong in certain individuals. To hurt the offender cruelly may become for them a dominating "ought." We do not approve of this. Ethics subjects requiredness in general to some principle of selection according to which certain things must secondarily be rejected that were primarily objects of positive requiredness. Again, ethics may ask us to do certain things which have primarily and as such a negative "valence." Our analysis remains on the primary level. I even have the feeling that it might be a good exercise to examine things on this primary level before we approach the tremendous task of systematic ethics.

On the other hand, such a descriptive procedure will be termed by many "mere speculation," mainly because it leads to unfamiliar results. Positivistically inclined scientists, for instance, will not like to hear about "dependent characteristics" which belong exclusively to structures or contexts. Moreover, they may admit it as a fact that the subject accepts some parts of his environment, and that he objects to others. If I say, however, that this fact implies transcendence—often over a great phenomenal distance—from the self to the object in question, and that acceptance or rejection is tantamount to such curious transcending reference, the same critics might be greatly disturbed by so much "mysticism." I dare not imagine what they might think of such transcendence as was here said to pass even beyond the phenomenal realm. For two centuries now Positivistic thinking has made us lay all stress on static self-contained contents of experience plus relations taken in an abstract and indifferent sense, while little attention is given to our awareness of specific "belonging" and of specific "references." The result is that we became unable to settle even such an elementary dispute as that between Phenomenalism and Realism (*Cf.* ch. 4). And for the same reason it appears to our time as a paradox that in a world of facts there are everywhere values. All science and philosophy is occupied with finding a system of concepts that make our experiences understandable. How can this attempt be successful, if Positivistic thought refuses to recognize some of the most essential and general characteristics of experience? It may be that Positivism once meant a laudable tendency to admit only concepts whose meaning could be traced to definite experiences. Long since, however, Positivism has become a doctrine in which only such experiences are freely admitted as belong in a particular class, the class of "mere facts." In other words, Positivism knows now what the constitution of the world *must* be; it represents in this sense a dogmatic attitude.

It will facilitate the following discussions if we now apply the same procedure to the concept of force as has just been used in the case of requiredness.

It may be repeated over and over again that from an epistemological point of view a force is merely an auxiliary concept which we find it convenient to use in the mathematical treatment of physical problems; when we think about concrete physical situations we shall none the less think of forces as vectors which actually accelerate or retard displacements. I do not believe that it will be possible really to eliminate this *dynamic* view of physical events. And, as a matter of fact, I am not convinced that we have any good reason for trying to do so.

At any rate, for our present purpose it seems to me advisable to think about physical facts in dynamic terms just as the physicists usually do it. In this sense I shall now attempt to indicate the meaning of the concept "force." Properly speaking this term cannot be defined. To be sure, it is easy to say how the intensity of a particular force is to be measured. But in measuring a force we take the meaning of this term for granted.[4] Ultimately a phenomenological source of the concept "force" must be given. And it must then be shown to what extent we are entitled to ascribe anything like this phenomenal datum to the physical world. For the moment it will suffice if we point out in what manner the physicists—officially or unofficially—use the term "force." This limits the possibilities of our analysis. Just as in the case of requiredness we can only enumerate characteristics of such situations as contain the factor of which we are speaking. It is in the nature of this task that the outcome of our description will at first appear as trivial. After all, we have no desire to make new discoveries about forces; we wish merely to see dynamic situations in such a light that a comparison between the concept of force and that of requiredness becomes

[4] *Cf.* ch. 5, pp. 147 ff.

possible. The following propositions may assist us in this task.

First: An entity is subjected to a force *within a context* of other entities.

Secondly: Within a context a force is a *dependent* characteristic in the sense that it has no existence of its own apart from the entities between which it operates.

Thirdly: Forces *transcend* from certain parts of a context to others of the same context. They are translocal directed traits of contexts that cannot be split into bits which have a merely local existence.

Fourthly: In the very meaning of the term it is implied that a force points beyond its present existence to a change which it is about to bring forth Whether or not such a change will actually occur, the "tendency" toward it cannot be eliminated from the concept force without destroying its sense. Forces either *further* the formation of a context or they *resist* it. In the first case they also resist the dissolution of the context; in the second they further its dissolution. Therefore they also further or resist the approach of new parts which are brought into a given context. When contexts are in a state of equilibrium, forces tend to maintain this state.—Both the positive and the negative operation of forces may occur in all degrees of intensity. Thus, as regards forces, there may be besides positive and negative cases also indifferent situations.

The direction in which forces tend to change given contexts has been discussed in the last chapter. It seems advisable once more to remember what was then said. If only two objects constitute the system in question this direction can generally be described in the very simplest terms. In this case forces tend to shorten the distance between the objects, i.e., to make their functional relation

more intimate, or they tend to increase this distance, i.e., to weaken their functional relation. In more complicated cases, we found, science has no such direct way of indi cating in what direction the forces of a system tend to change the system. This is due to the fact that under such circumstances there is not merely one force that operates between two objects, but a pattern of forces that act in different directions of space. In each of the possible configurations, the physicist would have to say, a system contains as a whole a certain amount of stress or, to use a more technical and correct expression, of potential energy.[5] The pattern of forces in a system tends to lessen the amount of this energy. The same rule—the Law of Dynamic Direction—holds of course also in the simpler cases in which forces tend to produce either an increase or a decrease of the distance between two objects; because such effects, too, are invariably associated with a reduction of available potential energy. The very generality of this principle involves, however, a great weakness in that it gives the principle a high degree of abstractness. What changes of actual configurations are brought about when the potential energy of a more complicated system decreases? We have no well-established theorem that expresses the direction of such changes in terms of configuration rather than of potential energy. As a single exception Mach's principle may be cited, which states that, when a condition of equilibrium is approximated, forces tend to give the system the most regular configuration of which it is capable.

[5] May I repeat that here as elsewhere in this investigation I am not referring to electromagnetic and electrodynamic events to which the concept of potential energy cannot be simply applied These events are of no interest in our present discussion—I am also ignoring all physical situations in which, besides forces, mere inert velocities determine actual displacements. These situations are again irrelevant to the following argument.

In Mach's formulation, however, the term "regularity" is not clearly defined; it is, therefore, difficult to apply the principle to configurations which are not "regular" in an immediately obvious sense. It remains nevertheless true that—whether or not we can describe it in clear configurational terms—there is a general direction in all transformations which the forces of given systems tend to realize. Thus, if a system is not yet balanced, its actual configuration, including the pattern of its forces, indicates the particular direction of those imminent changes by which the forces tend to make it better balanced.

To what extent do we "understand" the behavior of forces? If we know the parts of a context between which forces are active, and if we also know what properties these parts possess, the nature of the forces in question is thereby given. It is strictly determined by the nature of those parts; so that we can, for instance, predict in what direction the forces will operate, whether they will tend to make the given context more intimate or to dissolve it, and so forth.[6]

To be sure, the elementary laws which thus connect given materials with certain forces are at the present time entirely empirical, i.e., they cannot be deduced from any other knowledge. Most physicists will even be inclined to think that in this respect no further progress of science is to be expected. We do not know why an electron resists the approach of a second electron, or why an electron and a proton will attract each other. It is at least possible that we shall never know.

On the other hand, once the association between elementary materials and elementary forces is given, the more particular configurations which the forces will tend to realize can generally be deduced from such knowledge and from the cir-

[6] The medium through which the forces act may, it is true, exert an influence on the intensity of the forces between two objects However, this fact means merely that there is a primary interaction between those objects and the medium *Cf.* the "polarization" of a non-conductor by which the electrostatic force between two charged objects is altered. Here the "medium" itself belongs to the "parts of the given context."

cumstances which obtain in concrete cases. If, for example, the molecules of a liquid attract each other more strongly than do these molecules and those of a second surrounding fluid, it follows immediately that such forces will tend to give the common surface of the liquids the smallest possible area. —Again, the Law of Dynamic Direction is valid whatever special material and, therefore, whatever forces a system may contain. Indeed, the Law of Dynamic Direction may simply follow from the very concept of forces, as this concept is used in physics. In this case the law would of course be independent of those particular empirical rules which indicate what special forces operate between particular materials.

II

We are now acquainted with the way in which requiredness appears as a characteristic of certain phenomenal situations; and we also know about the manner in which forces play a part in the make-up of physical situations. These formal descriptions enable us to compare one case with the other, the concept of requiredness with that of force.

First: Just as something is phenomenally required within a context of other things, so a physical object is subjected to a force in a context of other physical objects.

Secondly: Just as requiredness is a dependent characteristic that has no existence apart from the data which fit or do not fit each other, so no force exists apart from those entities between which it operates.

Thirdly: All requiredness transcends from certain parts of a context to others of the same context; and this is also true of forces. Both requiredness and forces are directed translocal traits—of their situations—that cannot be split into bits which have a merely local existence.

There is little in these three points that does not hold

for all kinds of reference in general. It is the *fourth* point by which both requiredness and forces are more specifically characterized: *Requiredness* means that a given context either accepts its own constitution and the nature of its own parts, or that it rejects some phase of this given status. Requiredness is either final and complete, or it demands such changes as would lead to a more completely acceptable state. It may also assume the form of objection to such changes as make requiredness less complete.—The *forces* of a physical context maintain the status of this context, the configuration and the nature of its parts, if its constitution is that of an equilibrium. If a context is not balanced its forces constitute a pressure in the direction of balance. On the other hand, forces resist such changes as lead away from balance.

Thus, requiredness and forces resemble each other in one further respect. Both are "positive" or "negative" with regard to the status of those given contexts in which they occur. To this extent there is in the nature of both something that "mere" facts do not exhibit. More particularly, there is correspondence between final acceptance in the case of requiredness and maintenance of a balanced state in the case of forces. There is also correspondence between the positively demanding nature of requiredness, that points toward more completely acceptable states of phenomenal contexts, and the positive tendency of forces that point toward more completely balanced physical configurations. Lastly, there is correspondence between the objection involved in negative requiredness and the resistance which forces oppose to the formation of certain physical contexts.

A moment ago it became apparent that, quite apart from their thoroughly dynamic nature, requiredness and forces

occupy structurally identical positions in their respective contexts. So much followed from an examination of their first three characteristics. This structural resemblance extends, however, to the *dynamic* characteristics of demands and forces. To repeat: We find that "dynamic maintenance within a balanced state" is the homologue of "acceptance within a phenomenal context"; that the "positive pressure" of forces which point toward increased balance is the homologue of a "positively correcting demand" in the phenomenal world; and that the "resistance of forces" against the formation of certain configurations is the homologue of "negative requiredness" or "rejection" in experience.

When enumerating the characteristics of requiredness I mentioned that from one instance to another the degree of *insight* which we have into the foundations of demands may vary considerably. In the case of forces a somewhat similar situation seemed to obtain, inasmuch as sometimes forces clearly represent the very nature of those materials between which they act, while in other cases forces do not seem quite as sensibly connected with the properties of the materials. It is probably too early to compare requiredness and forces in this respect. A more thorough phenomenological study of value-situations and a more complete analysis of macroscopic physical states appear to me as strongly needed for this purpose.

The challenge which our present situation contains will become more urgent if we compare requiredness and forces not merely in general and *in abstracto,* but in cases in which they appear as directly and concretely *associated.* For this purpose we shall now consider phenomenal situations that contain the factor of requiredness, and we shall try to construct the cortical correlates of such situations.

Since we wish to discuss a matter of principle, any simple example will serve our purposes.

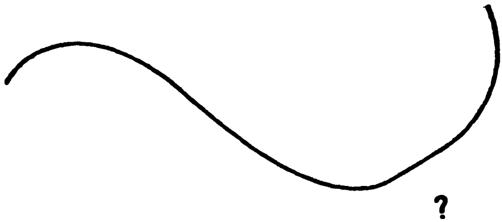

Fig 3

The reader will probably not find that the curve in figure 3 is entirely "right." The curve in figure 4 is "better." In the first curve it is the region indicated by a question-mark which appears objectionable in the visual context as a whole. So far we have merely confirmed what

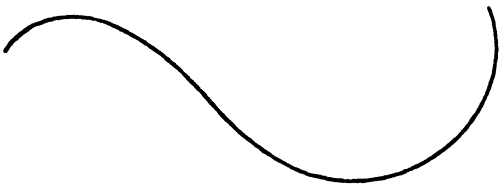

Fig 4

we knew beforehand about requiredness as a dependent characteristic of certain contexts and about its transcending character: The objection issues from the main parts of the curve and is directed toward that particular region. For many observers there will be a correctionist tendency in this objection. Not only is that particular region "wrong"; it "ought to be changed" in a definite fashion.

So far as the author's visual field is concerned, all parts accept each other in the second curve.

A curve in the visual field is a segregated figure. If we adopt the theory that was given in the sixth chapter, the cortical correlate of the curve is a macroscopic process. This process is segregated from the environment by the difference between its own chemical properties and those of the environment. In consequence of this the same circumscribed process assumes at once a particular electric rôle.[7]—Phenomenally, the gross structure of the curve makes one particular part of it appear as wrong. Whatever one may think of isomorphism in other respects, one will naturally assume that there is at least one correspondence between this rejection in the visual field and its correlate in the striate area: What I called the gross structure of the curve as a percept is neurally represented by certain parts of the "cortical curve"; similarly the wrong part of the percept-curve has its counterpart in a particular part of the neural curve. Where, then, shall we locate the correlate of an objection that the former parts raise to the shape of the latter? Somewhere else in the striate area? Between cortical processes the phenomenal counterparts of which do not object to each other? Certainly not; for once isomorphism becomes almost a necessity. Everybody will postulate that, if the rejection of one part of a percept by other parts is at all cortically represented, this correlate of rejection must be a neural fact which extends between the homologous parts of the percept-process. One will also grant that this correlate of phenomenal objection "does something" about the correlate of the wrong part with reference to the correlate of the gross outline. There is, however, only one kind of physical fact that represents the

[7] *Cf.* ch. 6, pp. 213 f.

characteristics of physical states in their environment and can thus "do something" about other physical states. These facts are fields of force. It follows for our present instance that only forces could occupy the same structural position in the neural field as requiredness occupies in the corresponding phenomenal configuration.

The example which we are now considering is one of *negative* requiredness, of *rejection.* According to our general analysis, "resistance" and "pressure against" parts of a given physical context correspond structurally to rejection in experience. This would apply to the present psychophysical problem, to the relation between rejection and its neural correlate, if we had reason to expect that in this instance forces really act as a pressure against the wrong part of the curve or, rather, against the neural correlate of this part. There is, indeed, one obvious reason for making just this assumption. In the main outline of the curve a simple regularity, an even flow, is indicated. This regularity is disturbed by the part to which, phenomenally, the other parts object. The present example belongs, therefore, to those cases to which Mach's principle refers: Forces act in the direction of more regular and more even configurations. Any forces that the cortical situation may here contain should thus assume the form of a pressure against the disturbing part. Actually we find not only that such a pressure within the neural substratum is to be expected as the counterpart of objection in experience. Requiredness has in our example a correctionist tendency. On inspecting the curve we see that the disturbing part ought to be rounder, that it ought to bend farther downward. Precisely this is the direction which pressure must have in the neural correlate of our figure if, in ac-

cordance with Mach's principle, this correlate is to tend toward a more even shape.

I do not wish to burden this discussion with technical matters. But a few words may be needed about the nature of those forces which we assume to operate between the parts of the "cortical curve." This curve is pervaded and surrounded by an electric current, and the distribution of this current depends on the shape of the curve (*Cf.* ch. 6, p. 214). Such a current, however, represents not merely an electric displacement. At each point of the current there is an electrostatic field that maintains the electric flow. As a matter of fact, the current and the field have everywhere the same direction, and the intensity of the former is at each point proportional to that of the latter. We are thus entitled to speak of a self-distribution of electric forces as well as of a distribution of current. One does not occur without the other. Now, it is well known that a pattern of electric force represents a store of potential energy. This energy tends to decrease. It would do so if the distribution of both the current and the forces were to become more regular. The only way in which this could be achieved is a change in the shape of the curve. Consequently the forces will exert a pressure on the curve, more particularly on those of its parts which prevent a higher degree of regularity and of balance.[8]

We need not at this point repeat our general comparison of requiredness and of forces in detail. It would now be a comparison between a particular form of requiredness as observed in a concrete perceptual situation and the rôle which forces play in the neural correlate of the same situation. Just as requiredness transcends as a form of reference from the main outline of the curve to a particular part of it, so the pressure in the underlying substratum represents the relation that obtains among the correlates of those same parts. Just as requiredness is a dependent

[8] *Cf. Die physischen Gestalten,* etc., pp. 251 ff.

characteristic in the perceptual situation, so the pressure is a dependent trait of the corresponding neural substratum. Requiredness appears in this case as objection to a part that does not fit the other parts of the percept; accordingly the pressure is exerted upon the correlate of the same part, because in its relation to the other parts it makes a more balanced distribution of forces impossible. In the visual field it is indicated in what direction the curve must be changed if it is to become entirely acceptable; in the neural substratum the pressure points to a more balanced configuration. And the direction which is indicated in visual experience coincides with the direction toward which the pressure points in the underlying substratum. In every structural respect the forces occupy in the neural situation the same position as that which requiredness occupies in the phenomenal situation. We may draw the conclusion that in the present perceptual situation requiredness has a neural correlate, and that this correlate is the tension which the neural substratum of the situation contains. It appears that in our example requiredness and its neural counterpart are *isomorphic.*

With our present neurological knowledge we can think much more clearly about the correlates of visual experience than about those of any other psychological facts. We could for this reason discuss many more examples in which requiredness belongs to the data of the visual field. In all these cases, however, the construction of neural counterparts of requiredness would lead to the same conclusion: We should find that forces play the same rôle within the macroscopic cortical processes as that which various forms of experienced demands assume in the corresponding perceptual situations. As these demands vary from one situation to another, so the behavior of those

forces would be found to vary in the neural substratum.

I refrain from considering such instances, because they would teach us little that is not contained in our first example. It seems preferable to choose as a second paradigm a case of *subjective* requiredness. I do it at the risk of making somewhat less definite statements about the neural correlate of the situation to which it refers.

Among my favorite colors is a very dark green. Some coniferous trees in America exhibit this color; occasionally I have seen it in a woman's dress. I cannot explain why just this hue should look so delightful, but it does; it attracts me strongly. This, I suppose, is an example of 'subjective' requiredness.[9]—We may take it for granted that the neural correlate of the green is a chemical reaction in circumscribed parts of my visual cortex. If I fixate the object in question these parts would be the occipital poles of the brain, because these correspond to the foveae. We are less well informed about the neural correlate of the self. To be sure, the self is to some extent represented in the visual field; but in several respects this visual self has a merely secondary importance, and it is certainly not this component of the self that feels attracted by the green. Another component belongs to the tactual and kinaesthetic sphere. It has a more central position in the constitution of the self; but I doubt very much whether it is this part on which the color primarily exerts its influence. I doubt it because I believe that the James-Lange theory of emotional states contains only a modicum of truth. It seems quite possible that our affective states are not specifically related to any sense modalities. My predilection for that color, on the other hand, certainly has something

[9] The term 'subjective' has here the purely phenomenological meaning that was indicated in ch. 3.

to do with my emotional life. We know that the visual representation of the self must be located in the striate area. The neural correlate of the self in so far as it is tactually and kinaesthetically perceived will be ascribed to the posterior central gyrus. No attempts to localize the neural substratum of a person's emotional states seem as yet to have passed beyond the stage of guess-work. To whatever parts of the brain further research may actually attribute these affections, experience points to the most intimate functional connection between the various cortical components of the self. Although many sources contribute to its make-up, the 'subjective' part of the phenomenal field, including the emotional life, the kinaesthetic and the visual components of the self, represents under normal conditions a unit which as such has commerce with the 'objective' world. We are thus forced to postulate a similarly intimate organization and centralization of all the neural events which underlie the phenomenal self. And as the phenomenal self generally represents one entity in its commerce with the 'objective' world, so its complex neural correlate will behave as a unit in its functional relations with the correlates of 'objective' percepts. Any ideas about the neural substratum of 'subjective' requiredness which we may develop will, therefore, refer to the fact that the complex neural correlate of the self forms at different times the most varied functional contexts with equally varied correlates of 'objective' percepts.[10]

It seems unlikely that requiredness has one neural substratum in one case and an entirely different correlate, or none at all, in another case. In fact, once the self and a

[10] Besides the correlates of 'objective' percepts, those of images and concepts may of course become partners of the neural self in such contexts. We need not assume that our problem is in this case entirely different from that which a perceptual situation offers.

color in the visual field are considered as parts of a context in which the color appears as a good or delightful object, and in this sense as required, all further reasoning will necessarily follow the course which we pursued in our discussion of the first example. It does not matter whether we say that the color "does something" to the self, or that the self "does something" to the color; in either case we shall have to assume that the neural correlate of one "does something" to the correlate of the other. This can happen only if forces act between the neural self and the neural color. These forces, I suggest, owe their origin to the relation which obtains between the neural self, as it is constituted at the time, and the nature of the neural color. In the present instance the action of these forces would tend to make the functional relation between the substrata of the self and of the color more intimate, just as in phenomenal terms the "acceptance" of the color shows a slightly correctionist tendency, in that I "feel attracted" by this visual object and may tend to approach it. I cannot say why between my neural self and this neural color precisely such a dynamic relationship should obtain; or why a grey, for instance, should be a "neutral" color when compared with most hues. And for the time being my ambition does not reach so far. I wish to point out merely that as a matter of principle a correlate of 'subjective' requiredness can be constructed, and that this rôle could be assumed by one single class of physical facts, which is the class of forces. If forces are the neural counterparts of 'subjective' requiredness, they occupy the same position in the correlate of the given situation as that which "acceptance" and "attractedness" occupy in this phenomenal situation itself. In other words, such forces represent 'subjective' requiredness isomorphically.

In a previous analysis I called attention to the fact that objects may acquire particular characteristics when they become the goals of demanding vectors (*Cf.* ch. 3, pp. 79 f.). It might be asked whether forces in the neural substratum could also change the characteristics of those correlates on which they act. There is no question about this; they can. We could solve this part of our problem with little difficulty. Its discussion would, however, lead us into so many technicalities that I prefer not to undertake it here.

The color of our last example could be replaced by many other percepts which, of course, need not appear just in the visual field, and which may exhibit all degrees of complexity. That we remain entirely indifferent to percepts to which we attend at all is probably an exception; that requiredness of some form or another accompanies their appearance seems almost the rule, although our positive or negative attitudes have for the most part little intensity. Many percepts, it is true, would not be strongly valued if it were not for acquired meanings with which they have been imbued by previous experiences, by learning in the widest sense. Neurally such meanings may have locations which differ from those of the percepts to which they phenomenally adhere. It is none the less obvious that in many cases the functional connection between the correlates of the percepts and those of their acquired meanings must have a degree of intimacy that makes them units for most practical purposes. In this sense, through previous experience, percepts may secondarily become value-objects in their relation to the self, and percept-correlates may become more than *percept*-correlates. Once this is granted, the neural theory of subjective requiredness will apply to such hybrid-percepts in their relation to the self, just as it applies to percepts which are as such attractive or repulsive. At least, if any new questions arise

in this connection, it is the theory of "associations" rather than that of requiredness which will have to deal with these problems.

Not only percepts as such and percepts with acquired meanings, but also concepts, other ideational contents and symbols with their meanings are—or become—positively or negatively required. A psychophysical theory of demands will have to be applicable to all these cases or to none. It is the very basis of human motivation with which we are now concerned. Food and drink, flowers and money, books and music, landscapes and persons, a nation or a principle, determine our behavior inasmuch as their presence, or our thought of them, involves that of corresponding demands—which may issue from their side or from that of our selves. Obviously there are no drives, urges or "instincts" that do not belong to our present topic. In this situation it will simplify the presentation of my point of view a good deal if I may give it once more a somewhat blunt expression: In a metaphorical fashion the springs of human action have often been called "forces." It appears that, if these springs have any counterparts, these counterparts can only *be* forces in the strict sense of the term. On the other hand, if they are actually forces, their behavior within contexts of neural events will resemble human motivation to such an extent that I doubt whether structurally and functionally any difference will be left.[11]

[11] I have not been able to see quite clearly to what extent this view differs from Professor K Lewin's ideas (K. Lewin, *Principles of Topological Psychology*, 1936, p 79 ff). When Lewin speaks of psychological dynamics he refrains from referring to the brain and from using the language of physiology or physics. I feel sure, however, that in the further development of the present theory Lewin's important discoveries will play an essential rôle.

I should perhaps add a short remark that will serve to prevent a misunderstanding. According to this view, forces within neural contexts determine our activities, but they certainly do not do it directly. Cortical tensions will often be unable essentially to change the neural configurations within which they exert their pressure. Neurally the self and a pleasant visual percept remain distant from each other even though forces may *tend* to make their functional relation more intimate. There is, however, one way in which these forces can achieve indirectly what they do not achieve immediately. In overt action the innervations of our musculature seem to depend on those patterns of macroscopic cortical processes which represent our situation at the time. If our assumption is correct they represent our situation in this sense, too, that both the presence and the direction of a demand have an isomorphic counterpart, a tension whose direction corresponds to that of the demand. It seems plausible to assume that the energy of such tensions is normally spent in stimulating motor areas of the cortex and in thus bringing about overt action. What would follow? With overt action the situation of the organism in its relation to its objects will change. As this situation changes, the pattern of cortical processes must change, because it pictures that situation. If, therefore, innervation follows those directions which are indicated in the cortical tensions, these tensions will now, *via* overt action, be released. In other words: Neurally the self and its goal will approach each other if the organism moves toward the object in question. But the organism actually moves and, I assume, it moves in this particular direction, because neurally there is a tension which has the corresponding direction between the self and the goal. This tension is spent in "steering" overt action; in doing this it produces just the effect which was implied in its own direction.

I need not mention especially that this short story contains about as many unsolved physiological problems as it contains sentences. In fact, it is meant to emphasize these problems *as* problems.

I have yet to show how the present psychophysical interpretation of requiredness applies to those instances in which demands were found to *transcend*—in the more specific sense of the term—either from beyond the phenomenal field into this field, or conversely, from this field to hidden entities beyond. It was such observations that first called our attention to the fact that the behavior of forces resembles that of demands. If demands can both issue from and be fulfilled by entities which belong to the physical world, they must at the point of their origin as well as at their object or goal be adequately represented by physical vectors. These vectors, it appeared, must be forces. We then undertook a search for the neural counterpart of requiredness in general. This led to the conclusion that the correlates of experienced demands are forces, and that between these correlates and the demands themselves the relation of isomorphism obtains. It is not a difficult task to unite these two lines of thought: When requiredness transcends from or into the phenomenal field, into or from the realm of traces, it is as such a matter of direct experience. In the first case we are also aware of the fact that the demand which transcends beyond the phenomenal field *is fulfilled* by a hidden entity; we merely do not see yet by what specific properties of this entity it is fulfilled. In the second case the demand is clearly felt *to issue* from a hidden entity, although for the time being this entity remains inaccessible. If we are right in assuming that the neural correlates of demands are forces which act within neural contexts, transcendence in this specific sense must obviously mean *that such functional contexts need not be fully represented in phenomenal experience.*[12] The forces which they contain *are* so represented—as de-

[12] *Cf.* W. Kohler, *Die physischen Gestalten,* etc. p. 202.

mands. The fact that there is balance *is* represented—as our awareness of "fitting." Again, the fact that those forces depend on the particular characteristics of a trace *is* represented;—the hidden "right thing" from which or toward which the demand transcends is pointed at by direct implication. Lastly, some "materials" of the neural context *are* phenomenally represented;—in one case we experience the phenomenal datum (the "test-object") that does or does not fulfil a demand from beyond; in the other case there is the incomplete phenomenal context from which the demand transcends to the hidden entity and accepts it. The only aspect of the neural context that has no phenomenal counterpart in these instances is the specific nature of its hidden member, the memory trace. Nobody knows under what circumstances a neural fact reveals itself in what we call a person's phenomenal field, and under what other conditions it is more reticent Any speculation on this topic would lie entirely outside the scope of this investigation.

III

The concept of forces has gradually become the center of our discussion. The way in which I speak of it is, however, not altogether in harmony with the connotation that the same term has for many people. The more popular view seems to be that when an object is subjected to a force it plays an altogether passive rôle. Unawares—if this anthropomorphic expression be admitted—the poor thing is pulled or pushed from one place to another. Similarly, other objects are driven through space by forces to which they have to submit as to foreign agents, and all these events taken together are "the facts of nature." This is not

an attractive picture; nor do we see how requiredness could have any place in it. Psychologists who are interested in the genesis of human ideas will probably find the history of this particular notion extremely interesting. But for our present purpose its only importance is that of an obstacle, of a misleading idea. When we speak of passive objects which submit to forces we ignore the fact that forces are characteristics of contexts, and that, if they operate on some objects, they also issue from others. These, the agents, would then appear to be just as active as the former objects are passive. As a matter of fact, no such distinction between an active and an entirely passive part is admissible in the description of dynamic situations. Whether any force operates between two objects depends on the characteristics of *both;* it is again the nature of *both* which determines whether such a force will tend to weaken their functional relation by repulsion or to make it more intimate by attraction. After all, the content of concrete observations is what happens to a context, not how an object is displaced under the impact of "a force" *in abstracto.*

Abstractness and vagueness of thought may in more than one way prevent us from realizing the actual import of the theory which has been outlined in this chapter. It will help us to see the theoretical situation more clearly if we examine the following question: Are we or are we not proposing one more kind of *naturalistic* thinking, if structurally and functionally we identify requiredness with what we call forces in physics? One might be greatly tempted to answer in the affirmative because of some such reasoning as this: Causation decides what happens in nature. But in each particular case causation is represented by the forces which operate in this case. It follows that

whatever happens in nature is due to the operation of forces. Conversely, since forces have no business except that of determining the events of nature, the concept "operation of forces" is coextensive with that of "facts of nature." On the other hand, requiredness was said to be in each case structurally and functionally isomorphic with forces that operate in the underlying neural correlate. It seems to follow that requiredness is practically identified with such "facts of nature" as constitute those correlates. And this proposition would surely amount to a particular form of naturalism. All facts which occur in a person's brain are causally determined; i.e., according to this reasoning they are due to forces. If requiredness as a phenomenal datum exhibits no structural and functional characteristics beyond those which are contained in accompanying neural *forces,* then it has apparently no characteristics beyond those of any neural *events* or, more particularly, of any neural correlates. Many such correlates, however, appear to be associated with mere psychological *facts.* Thus, there is no difference between the correlates of such psychological facts and the correlates of demands or values. From the point of view of isomorphism, therefore, no difference would obtain between mere phenomenal facts and requiredness. Indeed, how can the meaning of "ought to" be distinguished from that of "is" or of "happens," if "ought to," "is" and "happens" are all swallowed by one such general concept as causation? Actually, the swallowing might become a bit hard in the case of negative requiredness, of an "ought not" that opposes a fact. But this, it seems, serves only to show that the theory is not only naturalistic but also untenable inasmuch as it fails to do justice to an obvious observation. We do experience a difference between facts as such and demands which may or

may not be fulfilled by these facts. So long as this difference is not explained something must be wrong with the theory.

We cannot answer this criticism unless we know what we mean by *naturalism.* A theory of requiredness might be called naturalistic merely because it establishes a strict correlation between requiredness and some phase of nature. In this sense the present theory would rightly be called naturalistic. However, what we commonly understand by a naturalistic interpretation of value is something different. In this second sense of the term a certain assumption about nature is involved, namely, that it is a realm of mere existence and of mere facts; in other words, that nature exhibits no dualism that would correspond to the dualism of "mere facts" and "oughts" in experience. It is a naturalism with this premise to which objections are rightly being raised in logic, aesthetics and ethics. If we were to adopt this premise, and were then to establish a direct relation between values and something in nature, our theory would undoubtedly commit the very error that is attributed to naturalism; such a theory would be an attempt to reduce values to indifferent facts, and would thus contradict the very simplest phenomenological observations.

But is it under all circumstances wrong to search in nature for a counterpart of requiredness? Is it wrong, whatever view of nature we may have? It cannot be, unless we know *a priori* that nature must be a realm of mere facts, and that, therefore, it cannot contain any dualism which would compare with that of "facts" and "oughts."

When I tried to show that requiredness and the forces of corresponding neural contexts are isomorphic, I avoided the term "causation" altogether. My reason for doing this was that the general concept of causally determined events

covers many facts for which the forces of given contexts are not responsible. It is incorrect to say that "causally determined facts in nature" and "the operation of forces within contexts" are two expressions for the same thing. There is a dualism in nature no less than in human experience. And the constitution of nature itself bears little resemblance to that view of the physical world which both the naturalistic theories of requiredness and their opponents take for granted. In an abbreviated expression: The physical world itself is not "naturalistic"; because it contains, on the one hand, facts as such and, on the other hand, dynamic factors which either oppose or facilitate the occurrence of these facts.

This dualism will be apparent from a consideration of the Law of Dynamic Direction. Why do the physicists give so little weight to this law that they rarely formulate it expressly as a general theorem? Because they have decided to describe nature from the point of view of actual *events*. The Law of Dynamic Direction, however, may or may not indicate what really happens in a given system. It is primarily a law about the behavior of forces, not a law about the course of actual events. The direction which the processes of a system really take may differ widely from the direction which the law prescribes for the operation of forces. The reason for this difference is that inert velocities —for which the forces are not responsible—may be quite as much causal determinants of actual events as are the forces of the system (*Cf.* ch. 8, pp. 307 f.). Thus, real displacements may occur against these forces as well as in their direction. It is merely when sufficient friction eliminates all inert velocities that the Law of Dynamic Direction, the law of forces, becomes at the same time a law of actual displacements. Consequently we are not allowed to identify "the

operation of forces" with "everything that happens in nature."

There are no inert velocities in cortical processes. To this extent the present argument does not apply to psychophysical problems. But we have other reasons to distinguish between the operation of forces and causally determined facts in general. May I once more make use of a distinction which plays an important part in mathematical physics. Any particular dynamic problem in this science will be formulated in terms, first, of general laws that apply to this problem and, secondly, of "given conditions," i.e., of those concrete and established circumstances which happen to obtain at the time when the processes in question occur. Within certain limits these conditions may be arbitrarily given. More particularly, they may be determined by circumstances which have nothing to do with the forces of the system under consideration.[13]

This distinction can be directly applied to the central nervous system. The distribution of processes within the visual cortex, for instance, will be determined in part by forces which are inherent in these processes themselves. But their distribution depends quite as much on a given pattern of retinal stimuli. To be sure, this retinal pattern is not the only given condition on which the distribution of processes in the striate area depends. Strictly speaking we should mention as further conditions the given state of the nervous tissue in the visual cortex, traces that might exert an influence on present events, and even some further factors which are not located in the visual zone. For brevity's sake I shall not explicitly refer to these additional

[13] The reader will remember that the same concept of "given conditions" has been discussed in the eighth chapter (*Cf.* p 312).

circumstances. If I were to do so my argument could thereby merely become stronger. At any rate it will suffice if we restrict our consideration to these two factors: conditions which are by retinal stimulation and by nerve impulses imposed on the visual cortex, and forces which operate under these circumstances. The retinal conditions are established by the projection on the retina of points in the physical environment. In principle these mutually independent local stimuli may constitute any imaginable mosaic. Processes will now originate, and forces within these processes will lead to one organization or another, to this or that particular context. But there remains under all circumstances that pattern of imposed conditions, the nature and the composition of which were given from the outside. Thus, if neurally a visual situation consists of processes which are maintained by corresponding forces, these processes and these forces are not free to select the circumstances under which they operate. In other words, what actually *happens* in the visual cortex is one fact—it depends on *both* external conditions and operating forces; the distribution and the direction of *forces* as such is a second fact. It is only this second fact to which our comparison of requiredness and neural forces refers; and this second fact is not a "mere" fact. Processes have to assume a distribution which corresponds to those given conditions. But such a distribution may be well balanced in one case, much less so in a second case. Therefore, the forces which are inherent in these processes will either tend to maintain the present distribution, or they will point to changes by which a better balance would be achieved.

Obviously, then, we are justified in distinguishing between the actual distribution of neural visual facts as such and dynamic tendencies by which these processes react

upon their own configuration. In the example of the curve which we considered a while ago this point must be perfectly evident. Conditions of retinal stimulation are responsible for the fact that one part of the curve is incompatible with a better balanced state. Consequently there will be in the visual cortex a pressure against this part of the actual situation. In our theory the correlate of requiredness is this pressure, not the neural fact that owing to peripheral conditions the shape of the curve is not quite "even." Nobody, I believe, can fail to see that in this instance there is a dualism of neural facts as such and of forces which tend either to preserve or to alter these facts.

A few words will suffice to show that the same argument is applicable to the case of 'subjective' requiredness. We find ourselves in a friendly environment at one time, in a disturbing predicament at another. Unpleasant as well as attractive objects appear before us. Thus our selves often become members of contexts the other parts of which are given by external conditions. Such objects and environments will be valued, they will appear as satisfactory, as objectionable, and so forth. But we shall have to distinguish between their givenness, which may as such be a mere fact, and the positive or negative value-predicates, which they acquire in the relation to the self. The neural substratum of such situations exhibits the same dualism. In every case there will be a context which consists of the neural self and of the counterpart of an object or, more generally, of an environment. The presence of a particular neural object (or environment) in the neighborhood of the neural self is a matter of mere fact. But in response to this neural situation forces or tensions are likely to develop, and it is only these which our theory regards as

correlates of any 'subjective' valuation in the phenomenal field.

I shall of course not deny that the dynamic counterpart of a 'subjective' demand may become responsible for subsequent *changes* of the situation. If, for instance, a given environment does not appeal to the self, this negative valuation will be represented by forces that tend to change the neural environment or to remove the neural self from this neural neighborhood. Either in one way or the other relief may often be achieved, if the tension leads to corresponding action of the organism as a whole (*Cf.* p. 358).—It is also true that we often avoid situations before they are realized, and that we are in search of others which at the time are objects of mere thought. I should not hesitate to apply the hypothesis to these cases if more were known about the neural counterparts of ideational processes.

To summarize: It is not my contention that "causation" is the correlate of requiredness. Such a view would be open to all the criticism which has been raised against naturalistic interpretations of requiredness. If demands have any counterpart in the physical world, this counterpart must be something specific in nature which reacts, positively or negatively, to actual events or situations. And just as an "ought" may remain what it is, even though facts do not submit to it, so its counterpart in nature must point in an invariant direction, whether or not physical events take this course. Forces are isomorphic with demands in other respects. But they also fulfill this most essential condition.

Eddington occasionally hints at the possibility that the cortical counterpart of "purpose" might be processes which belong to the domain of entropy, the Second Law of Thermodynamics (*The Nature of the Physical World*, p. 105). The following argument may have led to this assumption: Physicists say that the Second Law is the only principle which in-

dicates in what *direction* physical events will occur. Purpose is conspicuous by its directedness. Thus the processes for which the law holds may appear as natural correlates of purpose. Eddington is one of the few physicists who clearly see how amazingly interesting the problems of psychophysics will sooner or later become for their science. In this case, however, I hesitate to accept his hypothesis. There is more "direction" in physics than that to which the Second Law refers. *Forces* are directed, and in the Law of Dynamic Direction—of which the physicists make little use—the general sense of this directedness is formulated. Under these circumstances it appears to be a more natural assumption that forces are the correlates of mental tendencies and purposes, than that diffusion, friction, or heat conduction play this part. These are the three processes which according to the present theory of entropy occur for reasons of mere probability, and which at the same time can occur in brain tissue. None of them actually has characteristics which would make it a plausible counterpart of purpose. It may be that Eddington himself would have assigned this rôle to forces in macroscopic dimensions, if it were customary to give as much attention to macroscopic *dynamic* distributions as is commonly given to distributions by chance.

CHAPTER X

Man and Nature

I

It is a philosophical task to indicate the place of value in a world of facts. The way, however, in which we have tried to accomplish this task has led through the domains of several sciences. We have been forced to propose solutions of certain problems with which these disciplines deal, and our final conclusions have a direct bearing on views which are commonly held by experts in the same fields. The sciences in question are psychology, neurology, other branches of biology, and physics.

I do not believe that in our present situation much could be gained by a further discussion of those assumptions which we introduced in connection with psychological and biological questions. Experimental research seems to me the only procedure that can decide whether these assumptions are fruitful in a pragmatic sense, and whether they are verifiable. This holds first of all for the hypothesis about the cortical correlates of percepts, which has been outlined in the sixth chapter. The view that any *particular* event in a sensory field will bring forth electric displacements by which its presence and its characteristics are indicated beyond its own locus must lead to consequences that can be tested. The same is true of the assumption that neural events register their own occurrence in the manner

that has been described in the seventh chapter. The more general import of both hypotheses is that histological microstructures have somewhat less relevance for the correlates of experience than is usually assumed at the present time. From a functional point of view, it seems to me, the cortex should be regarded as a quasi-continuum rather than as a special arrangement of linear conductors. In this case the principles of macroscopic field-physics will apply to the neural counterparts of phenomenal situations; brain processes will have to be considered in terms not of machine structures, but of general dynamics; and it will become a sensible procedure to compare directly the constitution of phenomenal fields with that of their counterparts, the fields of cortical processes. Taken in this sense, our hypothesis will be tested not merely by some particular experiments but by the course which psychology and neurology in general will take during the coming years.

With regard to physics I find myself in an entirely different predicament. There has been, I hope, no statement about physical facts and principles which a physicist could regard as wrong. On the other hand, I doubt whether any new factual consequences can be derived from those remarks in which I referred to this science. Thus it might appear as though there could be no dissension. And yet the physicist will be more likely to raise objections than any other scientist. Besides its knowledge of facts and principles, a science has its particular "spirit," its "set" or, as the psychologists in England would perhaps call it, its "sentiment." It is this curious psychological factor that more or less prescribes in what light the subject matter of a discipline ought to be seen at a given time, in what directions further developments are to be expected, and

what new points of view will be regarded with suspicion. I am unhappily aware of the fact that the attitude toward the physical world which I have taken in these chapters does not entirely correspond to the sentiment with which the physicists of our time look upon their field. This must have been obvious in the discussions of the fifth chapter; but there are further indications of heresy in chapters eight and nine. The main objection which a physicist might raise would refer to what may appear as the author's anthropomorphic tendency. I have been unable to find that the physical world is quite as different from the phenomenal world as it is now said to be by our greatest experts.

A strongly anti-anthropomorphic attitude began to develop in science as physics was transformed into a modern discipline. Aristotelian physics was at that time the great obstacle in the path of modern thought and of modern procedures. It is obvious that anthropomorphic notions contributed greatly to the stock of misconceptions which Galileo and others had to demolish before there could be any science in our present sense of the word. Nor were the physicists of the seventeenth century able to recognize to what extent even their own improved views were still imbued with anthropomorphic ingredients. Only generations of physicists could gradually discard or correct the more persistent influences which human subjectivity tends to exert on human science. The trouble is that for psychological reasons such a process cannot go on for generations without taking a turn that is not warranted by the actual situation. At first the task was clearly defined: Those particular ideas had to be eliminated which belong to the world of man but not to that of physics. Moreover, since man observes nature from a special position, such struc-

tural characteristics had to disappear from the system of physics as owe their origin to that special and—from the point of view of physics—accidental position. I have not the slightest objection to a program that declares war against narrowness. Unfortunately, however, even the best war, if it be prolonged, is likely to become a matter of "sentiment" rather than of circumspect measures by which certain evils are to be removed. When this happens our feelings tend to spread beyond their original and appropriate location. At first we are opposed to certain factors in an object which are clearly recognized as wrong. Gradually, however, we begin to feel an aversion toward this object as a whole whatever its other characteristics may be. In precisely this manner the physicists were at first right in their attempt to eliminate such influences of human subjectivity as were found to distort their views of the physical world. But in the course of time their suspicions lost this circumscribed localization and became directed toward man in general,—as though any human ingredient in science must necessarily mean a subjectivistic falsification of objective truth.

It would be difficult, I believe, to justify this attitude. Whenever a characteristic of human perception or of human thought is proved to disturb the objectivity of science, its influence will, of course, have to be checked. But does it follow that all qualities of the human, i.e., the phenomenal, world must be disregarded when the physicists construct their system of objective reality? Such a postulate could only be seriously considered if it were an established fact that between the phenomenal world and physical reality there can be no resemblance whatsoever. In the fifth chapter I have pointed out that, if this premise were granted, the task of physics would become insoluble.

There is no ultimate source for the physicist's concepts other than the phenomenal world. Moreover, the mere fact that a concept is derived from phenomenal data does not make it an anthropomorphic concept. To use an analogy: Astronomical theory should be independent of that particular astronomical object, the earth, from which we are compelled to make all our observations. But who would conclude that other planets, the sun, and other stars, must not be credited with any characteristics which the earth exhibits? Is it "geocentrism" to hold that chemically, for instance, all other objects of astronomy resemble the earth to a considerable degree? Similarly, there is no reason *a priori* why certain aspects of the phenomenal world should have no counterparts in the physical world. Any dogmatic or naive procedure which indiscriminately ascribes phenomenal traits to physical objects and events can obviously not be defended. On the other hand, it would be quite as dogmatic a procedure if we were to assume that between the former and the latter there can under no circumstances be any similarity. The sentiment, however, to which I have just alluded tends to make us believe that for a concept to be human or phenomenal is tantamount to its being "merely subjective" and unacceptable in physics.

To repeat: If this sentiment were definitely adopted, science would seem to be doomed to failure. At the present time we hear a great deal about the fact that any observation in physics involves some physical interaction between the observed objects and the observer, and that to this extent such interaction becomes a necessary factor in what we call "objective data." The observer, however, is quite as much a necessary factor in observational situations as is that interaction; without his presence there

would be no such necessary interaction.[1] Moreover, no observation is of much value in physics unless it be connected with other observations, and be thus interpreted. This connection and interpretation is a matter of thought. Even if the physicist's thinking assumes the form of mathematical processes, it remains thought, and is to this extent ultimately based on phenomenal material.[2] If we were to declare that this material is "merely human," and that therefore the physicist should not use it, we should declare at the same time that the physicist must give up his work altogether. On the other hand, any work that the physicist is actually doing will remain for us an entirely subjectivistic game so long as we submit to the influence of that sentiment against man in physics. It is of no use to demand that man be removed from the process which leads to physical knowledge. He cannot be removed. But if he is admitted, physics will be a form of knowledge whose ultimate concepts are of human, of phenomenal descent. With the now prevailing sentiment most physicists will deplore this fact; and yet for those who have such a sentiment there is no way out.

It will now be clear that, quite apart from the particular purpose of this investigation, I have had good reasons for pointing to certain resemblances between the phenomenal and the physical world. Inasmuch as some fundamental characteristics of both perception and thought may be assumed to have similar counterparts in nature, the unavoidable participation of man in the construction of physical reality loses all its threatening appearance. From

[1] It makes, of course, no essential difference whether the observer himself or a registering apparatus is in immediate contact with the events which are being "observed" In the latter case the observer himself will have to observe what happens to the apparatus as a physical object.

[2] *Cf* ch. 5, pp. 145 f.

this point of view it becomes the task of man as a physicist to construct the physical world in terms of those concepts which the physical and the phenomenal worlds seem to have in common. I cannot here discuss by what criteria such objectively valid concepts may be distinguished from others that would rightly be called "merely human." I wish to emphasize, however, that by our present discussion the situation from which we started has been completely changed. At first I had to defend myself against the charge of an anthropomorphic tendency. This charge was based on the fact that in our investigation physical and phenomenal facts were said to resemble each other in some essential respects. From the present argument it follows that physics would be hopelessly anthropomorphic if such a resemblance in essential traits did *not* exist. The constitution of the physical world *must* be described in terms that have ultimately phenomenal, and in this sense "human," meanings. If all such meanings belong *exclusively* to human experience, if none apply to physical facts at the same time, then physics will forever remain an anthropomorphic delusion. On the other hand, if human experience and physical facts have some basic traits in common, then physics can use experiential or human terms, and still be an objective science.

I may, however, seem to be guilty of anthropomorphic tendencies in a more particular sense. Precisely what, the physicists might ask, are those traits of phenomenal experience which should in one form or another be used in the construction of the physical world? Granting that there be some such characteristics, we are not for this reason compelled to accept what appears to us as unwarranted

speculation about an alleged affinity between a great many data of experience and the facts of nature. One of the principal points, for instance, which Galileo raised in his attack against Aristotelian anthropomorphism, was the wholly subjective character of "purpose" and other such teleological concepts. In Aristotelian physics objects were said to move "toward their proper places," just as human beings move toward shelter or toward any other goal. "To move toward" is an expression which seems to have some meaning when it refers to human activities. But there is no evidence that physical objects ever prefer one place to another, or that they have any goals. So long as physical facts were given such teleological interpretations, the need for explanations was much too easily satisfied. A scientific investigation of nature remained almost entirely impossible until physics learned to heed only those matter-of-fact dependences which are now commonly subsumed under the title of causation. Obviously, then, teleology was once the great obstacle in the path of science; and it would be about the worst thing that could happen to physics if anything like purpose were once again introduced among its concepts. Nothing short of this seems, however, to be attempted when requiredness is directly correlated with forces that operate in cortical processes. Requiredness as such is a notion which would disturb the sober neutrality of physics if it were ever admitted in this science. But in the ninth chapter even 'subjective' requiredness, "the springs of human action," i.e., human motives, were practically identified with physical tensions which develop between the cortical correlate of the self and that of an object. This, the physicist might say, means a more intimate relation between his concepts and those of psychology than his concepts can bear without an alter-

ation of their strict meanings. If there is any stronghold of such ideas as purpose, it is human motivation. We cannot speak of motivation without speaking of goals, and if we speak of goals our thinking will necessarily imply a teleological determination of facts. On the other hand, we did assume that structurally and functionally the behavior of certain cortical tensions is isomorphic with the play of human motives. Must one not conclude that, as a next step, it will be proposed that teleology be reintroduced into the system of physics? Undoubtedly, any attempt in this direction would meet with energetic resistance from all physicists who have not yet succumbed to certain metaphysical tendencies of the present time.

This criticism, I believe, can be answered quite simply. The principal objection against teleological notions refers to the fact that, for actually teleological thought, events which will happen in the future are capable of determining what happens now. It seems to me that if this be taken literally we have no evidence of any teleological determination whatsoever. And I wish to emphasize particularly that I see no more evidence of it in psychology than I do in physics. What, indeed, are the facts which have led to the thesis that in motivation the future takes part in the determination of the present? May I consider a very simple example. I expect visitors tomorrow, and in view of their coming I make certain preparations today. It cannot possibly be maintained that in this instance the future as such determines my present activities. This is impossible for the obvious reason that the future events in question may never actually happen. When all my preparatory measures are taken I may receive a telegram by which my friends' visit is cancelled. Nobody will be able to convince me that in this example a future event that will not

actually occur has none the less been capable of determining my present actions. I admit that *in some sense* the future had something to do with the preparations which I made today, just as it has in all cases of human planning. But what kind of future is it that plays this part in present planning? Those mental processes to which I refer when I say that my friends will arrive tomorrow are *present* processes. They occur *now,* not tomorrow. They are also determined by present or by past events, not by future events which have not yet occurred, and may never occur. Among the contents of my present phenomenal field there are, on the other hand, certain thoughts or images which I locate in what we may call *the now experienced phenomenal 'future.'* The now experienced phenomenal time has in this sense a wide extension. I may now think of an event that occurred three years ago. When doing so I remain in the present; and so does my thinking as an event. The fact that I feel referred to that 'past' event does not mean that I am now in connection with the fact which actually happened three years ago. It can only mean that some parts of the actual present have phenomenally the 'pastness'-index, just as others appear not in my spatial neighborhood, but at a great distance. Again, some mental contents to which I am now referred may have the index of 'futurity'; contents which belong to this class may also influence my present conduct, and, as a matter of fact, they do so continually. But we should not identify contents which have now a location in the phenomenal 'future' with anything that will or may actually happen at a later time. We do see many goals before us at a temporal distance; but this phenomenal distance of which we are now aware does not as a dimension coincide with those stretches in time during which our goals may or may not

actually be attained. To express the same distinction still more succinctly, although perhaps a trifle paradoxically: It is not the actual future, the future as such, toward which we are directed in our planning, and in which we perceive our goals, *it is that part of an actually present phenomenal field which we call the 'future.'* From the point of view of function and of causal determination this part of the field, and any goals within it, are no less present facts than is an ordinary percept before me. As such present facts they constitute the situation to which our planning and our preparations functionally refer. And although the actual future which we experience later may more or less resemble what we *now* think and plan about the *now* given 'future,' it is only this now experienced and not the actual future with which our purposes and our plans have any causal connection.

We may conclude that the actual future is at a given time just as non-existent and ineffective in psychology as it is in physics. The constitution of purposes, on the other hand, is merely that of vectors which extend from the now given 'self' to objects that are located in the now experienced 'future.' Thus, if human motivation was said to have an isomorphic counterpart in cortical tensions between the correlate of the self and that of an object, this statement can not legitimately be interpreted as though it meant the introduction of teleology into the realm of nature. Taken in the sense in which this concept would be objectionable in physics, it seems to me just as unacceptable in psychology. Surely, I do not wish to propose that forces are determined by what will or may occur at a later time; with the physicists I would contend that all forces operate in contexts which are fully given at the time of their operation.

One might here raise the question how given phenomenal fields can sometimes contain a phenomenal 'past' and sometimes a phenomenal 'future' as well as a 'present.' It seems to me that this is not a problem which we may hope to solve on purely phenomenological grounds. The phenomenologist, it is true, may do much valuable work in describing the properties of both the now given 'past' and the now experienced 'future.' But he will be unable to say why and how a phenomenal field can have such regions at all. As a matter of fact, if there is any such problem, it will have to be formulated in psychophysical terms. What, we should then have to ask, are the functional characteristics of those processes which underlie the now given 'past' and the now experienced 'future'? What is their mutual relation? And what is their relation to processes which underlie phenomenally 'present' data? I cannot answer these questions, and for the time being I shall refrain from adding further hypotheses to those which were proposed in previous chapters. In no case should I assume that the 'past,' the 'present' and the 'future' in this phenomenal sense are directly and simply related to the temporal properties of cortical events in the physical sense of the word temporal. However that may be, these problems are intensely interesting but they have no immediate connection with the question of teleology in mental life.

If in their fear of anthropomorphism many physicists will object to the view that certain physical forces within macroscopic cortical processes are the correlates of mental tendencies, valuations, and so forth, others will criticize the same assumption because the concept of force itself seems to them a thoroughly anthropomorphic notion. The neural counterparts of phenomenal experience, they would say, must be genuine physical facts; they cannot be merely further subjective phenomena which the layman wrongly projects into the purely mathematical concept of force.

In dealing with few other concepts are we as likely to lose our epistemological bearings as we are in the case of force. There is a habitual but, I believe, misleading line of thought that refers to forces, and seems to deprive this concept of all objective value. It has often been said that the word "force" derives its meaning exclusively from our experience of muscular contractions. I am by no means sure that this opinion is well founded; but for the sake of the present argument we may as well accept it. As many physicists think about such matters they will be inclined to add some such proposition as this: Being a muscular sensation, force is a human, a subjective phenomenon. And by their sentiment against man in physics they will then be carried to the conclusion that a force in this sense is *nothing but* a subjective phenomenon, that it can have no place in physics. Hence the tendency to reduce the status of forces in physics to that of a merely mathematical concept which has nothing to do with experienced forces.

This argument, I must confess, does not seem to me compelling. First of all, let us remember that, if we contract certain muscles, say, of an arm or a leg, we do not actually feel what happens in these muscles. Just as in the case of vision we do not experience what happens in our retina, just as in this case retinal processes bring forth cortical processes on which visual experience is based, so the contraction of muscles leads to certain changes in those cortical states by which at the time the position and the condition of our limbs are represented. It is these cortical changes with which our experience of force is correlated. The physicist might conclude that in this manner we merely emphasize the subjectivity of forces; because cortical processes are even farther removed from physical reality than are our muscles and their contractions.

We shall examine this proposition later. For the moment it will suffice to point out that in this respect there is no difference between our awareness of force and any other perceptual data; for they are all correlated not with events which happen between our organism and its physical environment, but with cortical situations. If, therefore, the proposition were valid in the case of forces, it would be equally valid in the case of all percepts without exception; they would all be too subjective for the physicist, none could be used for his purposes, and physics as an empirical science would become impossible.

The physicists, however, do not reason in this manner when they think of certain other percepts. Vision is the sense modality upon which they principally rely in their experimental observations. And vision, one might be inclined to believe, gives them a direct access to those external facts in which they are interested, while our awareness of forces refers even peripherally to mere affections of our organism. Here the psychologist will raise an objection. The eye, it is true, is often called a distance-receptor. This, however, is a most confusing expression. I admit that in visual experience 'objects' appear at various 'distances' from the visible 'self.' But both the 'objects' and their 'distances' belong to the phenomenal world. From a functional point of view, vision begins with the projection of retinal images upon the retina. It is a pattern of physiological stimulation on a sensory surface, not actual objects and actual distances in the physical world, from which our awareness of 'objects' and 'distances' evolves. These 'objects' and these 'distances' are quite as much the outcome of intra-organic events as are 'forces.' If we attribute a higher degree of objectivity to visual percepts than we do to our awareness of forces, the reason for it is

once again an ambiguity of our terms. Phenomenally visual things do appear 'outside,' phenomenally they do for the most part look 'objective'; but in this connection the word 'outside' does not point to a distance in physical space; the word 'objective' does not refer to objective existence in the physicist's sense; and against the strongest temptation we should never identify a visual thing with the corresponding physical object. Since this point has been so thoroughly discussed in the fourth chapter, I need not here enlarge upon the same topic. So far as their functional genesis is concerned visual objects are, in principle, no more objective than is a 'force' which I localize in my 'arm.' It is merely the phenomenally 'objective' appearance of visual 'things,' the phenomenally 'subjective' character of 'forces' that make us prefer the former, as though they were more objective in a functional and an epistemological sense.

There is, however, another argument that might cause the physicist to trust vision more than he trusts "the sense of force." Vision seems as a rule to be much more articulate than are our kinaesthetic experiences. And, although the functional relation between visual data and corresponding physical situations is not at all as simple as we are sometimes inclined to presuppose, it remains none the less true that in several respects a visual field can often be regarded as a picture of the physical situation. I cannot deny that this is true; because in the fifth chapter I have myself made much of this mascroscopic resemblance between the visual field and physical situations. To be sure, in kinaesthetic experiences, too, there occur certain data which may in the same sense be called veridical. But the mere fact that we tend to test such experiences by a good *look* at the objects in question, clearly indicates that we ascribe

a higher degree of accuracy to vision. Vision, however, is commonly believed to convey to us no data from which the concept of force could be derived.

I do not wish to deny that vision exhibits certain traits which make it a particularly valuable witness to some external facts. On the other hand, a sense modality that is for certain purposes superior to others need not be so in all respects. I doubt whether visual situations never contain anything like 'forces' or 'tensions'; since, however, *intense* manifestations of 'force' do seem to occur more rarely in visual fields than they do elsewhere, we shall continue to disregard visual 'forces' altogether. But what follows? Would it not be a careless procedure if in physics we were to rely on one sense modality exclusively? The danger that quite particular traits of human organization become predominant in our view of the physical world, and that this view be thus anthropomorphically distorted,—this danger will be greatest if we ask only one witness among our sense organs what it has to say about physical facts. And there is danger in several directions: A given sense modality may, for instance, *add* subjective ingredients to the story about physical situations which it tells us; or it may *omit* certain traits of these situations, because it is not able to represent them adequately in its language. In both cases it would seem to be an altogether necessary measure of caution that we compare the account which is given, for instance, by vision with the report of other senses.

In spite of its merits in other respects, it is quite possible that vision cannot give us a complete report of the dynamic phase of physical nature; or, at least, that in this respect kinaesthesis is far superior to vision. Of course, those cortical processes with which our percepts are asso-

ciated have never any direct contact with forces in outside nature,—whatever the ultimate meaning of this word may be. On the other hand, peripheral stimulation of one sense organ may be immediately related to such forces, while stimulation of another is not so related. Actually the second case is realized in vision. Suppose that a macroscopic physical situation contains intense forces. Even if these forces were the most essential parts of the situation, and as "real" as anything with which physics has to deal, how would such forces be represented in the pattern of stimuli by which that macroscopic situation is projected upon an observer's retina? The answer is that the forces would have no retinal representation whatsoever. Forces do not emit light waves; nor do they reflect light. Since light which is emitted or reflected constitutes the sole message which reaches the eye from a physical situation, it carries no report about the presence of forces within or between physical objects, however intense these forces may be. In this respect the stimulation of our muscles in action differs widely from that of our eyes. Often either the resistance of physical forces which we try to overcome is directly involved in muscular action—as when we try to break a board or bend a steel rod, or it is the pull of physical forces against which our muscles operate—as when we lift a weight, or try to remove a glass from a wet surface. The nature of those processes which are in such cases the neural counterparts of kinaesthetic experience is not known. But it would not be unreasonable to assume that these processes bear some structural and dynamical resemblance to the facts by which they are peripherally caused; e.g., an object which stimulates the touch-receptors, a pull exerted on this object by a physical vector, and a muscular tension, which balances or overcomes that pull. If visual fields

exhibit any forces, their functional origin must under all circumstances be quite secondary; in kinaesthesis, on the other hand, the behavior of physical forces takes often directly part in peripheral stimulation. The more 'subjective' sense modality may therefore, at least in this respect, give us a more complete and thus a more objective report about physical facts than does vision.

To repeat: I admit that even in the case of kinaesthesis the neural correlates of forces are not external physical forces which have somehow migrated from the peripheral situation into the cortex. But if an altogether immediate representation of external physical forces does not take place, what of other concepts which are freely used in physics although the phenomenal data in question must have neural correlates of quite as indirect an origin? What about the third dimension of space? What about filled and empty space? What about boundaries of segregated objects, and these objects themselves? And, last but not least, what about movements? If we disregard touch and kinaesthesis, because the physicists tend to ignore them, all these notions must be due to visual experience. And yet none of them is represented in peripheral stimulation of the eye. Movement, for instance, is not so represented, because the changing pattern of retinal stimulation that corresponds to the movement of a physical object before us might quite as well be caused by a number of appropriately chosen local changes in color and brightness, in which no physical locomotions are involved. Between these two cases there would be, retinally, no difference. Similar considerations lead to the same conclusion in the case of all the concepts which I have just mentioned. They are all due to an organization of visual processes which is a wholly intra-organic affair. If these parts of psychology were more

widely known, Formalists might become less critical of the *dynamic* interpretation of forces, which has a phenomenal basis; because the phenomenal data from which many entirely indispensable concepts of physics are derived have functionally just as subjective an origin as has our feeling of forces.

It may not be relevant in this discussion, but it should none the less be mentioned, that from the phenomenological point of view experienced forces are by no means always 'subjective.' When I bend a steel rod I may feel an 'effort' in my 'arm'; but I also feel a 'resisting force' which is localized in the 'rod.' Again, if I try to keep a piece of iron at a short distance from the poles of a strong magnet I may feel a 'force' which I have to exert; but at the same time this 'force' is felt to balance a 'pull' that is not my pull, but 'objective' and localized 'outside.' The reader will not maintain that such experiences can only be the product of previously acquired knowledge, and that in this sense they do not belong to "kinaesthesis" proper. Why should all percepts whose peripheral origin is muscular be localized in the 'muscles'? Visual percepts are not localized where our 'eyes' are. It is, then, not quite correct to say that vision gives us at least 'objects,' while kinaesthetic experience contains exclusively 'subjective' phenomena. If this point is here of any relevance we ought to realize that so-called kinaesthetic experience has its 'objective' side just as vison has, and that among its data there are 'objective forces' no less than 'subjective efforts.'

II

It may appear as though the last arguments were more likely to cast suspicion on all concepts of physics than to legitimate the particular concept of force. For this reason I shall now consider our problem from quite another angle.

The physicists say that they want to know about the physical world, not about man. This attitude is under-

standable only so long as we assume that man himself is an entity which has no place in the realm of physical nature. When reading certain statements of physicists one is, indeed, almost forced to conclude that these scientists regard man as a strange being, which an accident must have thrown from an entirely different world into its present physical environment. His characteristic traits correspond to his foreign descent; and anything may happen when this creature tries to study physics with such an equipment.

In a treatise on the theory of probability I recently read the following argument: The theory of probability has either an empirical basis, i.e., it is ultimately based on statistical experience; or it is a mathematical discipline *a priori*. The author rejects the second alternative; because the physical world is found to obey certain major rules with which the theory of probability deals. Why, he adds, should this be the case, if the human mind is capable of deducing the same rules by *a priori* reasoning? We cannot possibly assume that a preestablished harmony exists between the demands of such human reasoning and the behavior of physical systems. Thus he concludes that the science of probability develops from statistical experiences. The alleged *a priori* reasoning actually evolves from whatever random behavior occurs in our environment.

I do not wish to discuss this particular conclusion. For our present problem the form of the argument and its premises are more important than is its special content. The scientists are now so much in the habit of divorcing man from the world of physical facts that any affinity between his mental functions and the operations of this world appears to them as a strange coincidence, in which no sane person can seriously believe. The argument which

I have just cited could with slight variations be applied to the whole mental equipment of man which he has apparently not "learned" during his commerce with nature. If any of these autochthonous properties were to resemble or to correspond with any properties of nature, such a fact would be most astounding; it would constitute a case of preëstablished harmony.

In criticizing this point of view we approach the crucial part of our discussion of anthropomorphism in physics. Man does not come from another world; he is himself a child of nature, of that very nature which the physicists investigate. When we compare his characteristics with those of nature we must, therefore, *expect* to find resemblances. It is not these resemblances which should surprise us; on the contrary, any traits of man which have apparently no counterparts in nature will by their very existence constitute serious theoretical problems.

In the biological sciences we do regard man as a part of nature. If no miracles have occurred during his own development and during that of his animal ancestors, the human organism represents a physical system all the characteristics of which will sooner or later be traced to the constituents and to the fundamental laws of nature in general. It is a truism that no inanimate system shows anything like that tremendous concentration and organization of heterogeneous physical agents and physical processes which the organisms exhibit. No object or process which the physicists study combines in any comparable degree the fluidity and plasticity on the one hand, the stability on the other hand, which are so strikingly united in animals and man. And yet, if our adherence to the idea of evolution means more than lip-service, it must be our postulate that no factors, which take part in the constitution and

in the operations of man as a living system, are fundamentally of another essence than are the factors with which physics and chemistry deal. Much is obviously *new* in organisms in the sense that just such configurations and such particular functions as they exhibit do not occur in the physical world. As Evolutionists we shall, however, maintain that in the living systems general principles of physics operate under particular circumstances; we shall admit that the first occurrence of these special circumstances in the history of the earth is not yet explained; but we shall deny that life is governed by any principles which are as such essentially different from those of the inorganic world. This holds for the particular functions which are investigated in physiology; but, as we found in the eighth chapter, it seems also to hold for the "causal harmony" which characterizes the interaction of these functions in the organism as a whole.

Evolutionary thinking would, however, not be consistent if the same point of view were not applied to the structural and functional properties of mental life. Human *action* depends on these properties. The fact, for instance, that we often perceive relationships between objects leads to certain forms of behavior in which such a relational perception is implied. Again, parts of contexts may not only appear as right or wrong with reference to these contexts; human beings *act* over and over again in a way which is directly determined by such requiredness. According to evolutionary principles there can be no physical activity which may not be understood in terms of the general laws of nature. Inasmuch, therefore, as human activities reflect the structure of corresponding mental functions—which thereby influence physical facts—these mental functions themselves must be capable of interpretation in the same

terms. Otherwise the idea of evolution would not be applicable to all physical facts.

I do not propose that phenomena *qua* phenomena can be physically "explained." At the present time even the most radical Evolutionist will not be able to tell us how red and green and the other qualitative traits of the phenomenal world have originated. But the phenomenal world has two aspects: a first, in which it appears as full of *qualia* of one kind or another, and a second, in which it exhibits structures and functional relationships of various kinds. It is the second aspect of mental life which we see continually materialized or realized in overt actions of human beings. Whatever the final interpretation of mere *qualia* may be, the various types of mental function which lead to corresponding activities, and thus to changes in nature, must be subject to the principle of evolution. This means concretely that even the most important structural and functional traits of the phenomenal world must have a basis in nature. Such characteristics of mental life might be *new* in the sense in which, for instance, certain vegetative functions of the organism are new. But just as these new organic functions follow under certain conditions from general principles of physics and chemistry, so these important functional traits of mental life must be deducible from the same principles—even though in inanimate nature no directly comparable characteristics may be discovered.

It will be obvious that from this point of view the physicists' sentiment against man appears as a dislike of problems that belong to their own field. As a matter of fact, the physicists should welcome the decisive test of their concepts and principles to which the interpretation of mental functions and structures must gradually lead. They might of course refuse to accept the principle of evolution.

But if they adopt it, they can no longer regard man as an entity from another world that unfortunately plays a rôle in the development of physics, but whose participation should be reduced to a minimum. The very being that observes physical facts in the narrower sense of the word, that thinks about these facts, and that thus builds up the science of physics, constitutes at the same time the most challenging subject-matter of that discipline. It does so whether or not its mental operations happen to refer to the study of inanimate systems; because any trait of its mental life that is functional and functionally effective will have to be derived from facts and principles with which the science of general dynamics is concerned.

In this sense the physicist's hostile attitude toward man will sooner or later have to give way to a wider conception of his own task. His principal preoccupation with regard to man will then no longer be that of immunizing physics against the virus of human subjectivity; rather he will recognize that the functional and structural characteristics even of such subjectivity represent facts of nature which he must include in his system. This will happen when he begins to ask himself: What is the origin of those "subjective" characteristics in a world in which, so far as function is concerned, my data and my principles have a strictly universal significance?

In two ways this question will bring forth a revolutionary change in the physicist's outlook. From the point of view that is now predominant in science the mental constitution of man is vaguely suspect. It is the "other," the "different," the "non-physical" in contradistinction to the physical in the usual sense, whose disturbing influence on science the physicist dreads. He wishes to obtain an adequate picture of inanimate nature; but he doubts whether man, the "non-

physical" author of physics, will ever be able to furnish such a picture. If we assume, however, that in evolution no functional and no structural characteristics could develop which may not be derived from principles of general dynamics, then any characteristics of man that are involved in the creation of physics will fall under the same rule. Suppose the rule is right. In this case the physicist will not only consider from what data and principles of his own field the structure of human perception and human thinking may be deducible; as questions lead to answers he will eventually succeed in this enterprise. He will know precisely what dynamic events in man take part in physical research, and, instead of dreading an unknown "foreign" influence, he will now be able to pass well-founded judgments on both the merits and the shortcomings of the human "instrument." Theory and knowledge of this instrument will replace what is as yet no more than a dim and slightly sterile suspicion.

We may expect that still a further change will at the same time occur in the physicist's point of view. Physics has never simply conquered a new field. Whenever science seemed to absorb new facts, its own system underwent a certain alteration, because the inclusion of such facts gave a new appearance even to the store of familiar knowledge that had previously been gathered. When Maxwell showed that the facts of optics can be derived from the laws of electromagnetism, something happened of course to optics. But neither did electric and magnetic fields remain quite the same concepts when it became apparent that in a state of oscillation they constitute light waves. In the same manner, I believe, the inclusion of man in the system of general dynamics will give this system a novel appearance. This may happen, first, when principles of dynamics are

applied merely to such mental operations as participate in physical research. Secondly, however, a similar, and perhaps a stronger, reaction on the system of physics may take place when mental structures and functions in general become a subject of dynamic interpretation. After all, from the point of view of evolution there is no reason why only those particular mental operations should interest the physicist. Any mental function, including those which play at present no perceptible rôle in the physicist's work, will have to be considered in the same light. Some such functions are likely to differ considerably from all dynamic events which are known to occur in inanimate systems. They will, therefore, represent a new material whose absorption into the system of physics may not be possible unless this system itself be slightly readapted. Every major expansion of physical theory, I repeat, has had the same effect. It would be astounding if this greatest annexation of new territory were to happen without such a repercussion.

At this point it seems advisable to indicate more clearly in what sense an interpretation of mental facts in terms of general dynamics is to be understood. To the extent in which mental structures and functions are as such phenomenologically observable they will surely never be "reduced" to anything else. The principle of evolution, on the other hand, demands that a dynamic theory of the organism be given in which we advance step by step from ordinary physics to such events as have the structural characteristics of mental operations in all their various forms. This is partly a task of physical theory as applied to the conditions which obtain in the nervous system of man, partly a task of physiological research which enlarges our knowledge of those conditions, and which at the same time examines the

theorist's constructions. These constructions will reveal to what extent that postulate of evolution can, as a matter of principle, be fulfilled in terms of general dynamic theory. Physiological and neurological research will have to show whether the same postulate is actually fulfilled, and if so, whether it is fulfilled in the way which the theorist predicts. The reader will realize that with this program, which follows from the principle of evolution, we return once more to a concept that played an important rôle in previous chapters. The principle of evolution postulates that certain processes of which the organism is capable have the structural characteristics of mental operations. If there are any such processes, they must obviously be the neural correlates of these operations. Thus mental operations and their neural counterparts must structurally resemble each other. In other words, *the principle of psychophysical isomorphism follows from the principle of evolution.* Isomorphism represents, indeed, the only way in which mental life can be dynamically interpreted, in which it can become a subject-matter of physics. In this sense we attempted a dynamic theory of mental facts when we pointed to certain properties of macroscopic dynamic states which appear as isomorphic with characteristics of mental facts, and which seem likely to be the neural correlates of these mental characteristics: the continuity of the visual field, the segregation of circumscribed entities in this field, the topological relations in phenomenal space, the more "material" relationships between phenomenal objects, and eventually, requiredness.

Up to this point psychophysical isomorphism appears as a fruitful principle. If it should prove to remain so in further work we may hope to use it in a way which would constitute a new approach to the general problems of

dynamics. Physical theory and physiological research, I said, will try to subject mental operations to the principles of physics. Suppose, however, that psychophysical isomorphism should gradually become a reliable axiom,— as it must be if evolution is a sound axiom. In this case isomorphism could tentatively be taken for granted. So far as structure is concerned, the neural correlates of phenomenal experience would have to be strictly isomorphic with such experience, and consequently experience would give us a more direct knowledge of certain aspects of macroscopic dynamics than any physical or physiological approach could ever be expected to yield; because such an approach is indirect under all circumstances.

A moment ago I pointed out that a dynamic interpretation of mental facts might lead to certain alterations in the system of physics. It will now be fairly evident that such alterations are to be anticipated for a particular reason. In the selection of their concepts the scientists proceed with extreme parsimony. Their basic concepts are few in number, and any connotations of these concepts which are not strictly needed for the business of physics in the usual sense are weeded out with great care. From this point of view it seems merely human weakness if, for instance, the dynamic content of the concept "force" is, by the same scientist, treated with contempt during an epistemological discussion at 10:30, and freely used in thinking about concrete physical situations at 10:45. As a matter of fact, so long as the physicist remains a physicist in the narrower sense of the word, this may not be a serious issue, since physical evidence as such does not decide one way or the other, and since all physicists share the pragmatic advantage of the dynamic, the more than mathematic, conception of forces, inasmuch as they all sin at 10:45.

The parsimonious epistemology of physics will, however, have to pass an examination when the concepts of science are applied to physical contexts in the human brain, and when they are thus confronted with experience which, according to the principle of isomorphism, represents the most direct information about the same contexts. It may then be discovered that at one point or another the parsimoniously selected and defined concepts of physics are just a trifle too meagre, that isomorphism can be approximately, but never quite fully, attained if the strictly Puritan editions of these concepts are used in brain-theory, and that a complete congruence of mental with cortical structures will not be reached until the same concepts gain some weight and a slightly richer content. Evolution is a postulate which no scientist will easily sacrifice. On the other hand, evolution implies isomorphism. I feel inclined to predict, therefore, that in such a situation the concepts of physics will actually be enriched in a way that makes strict isomorphism possible. This, I believe, will happen to the concept of force, but it may happen to other physical concepts, too. The direction in which such changes will occur is by no means unpredictable. It will be strictly prescribed by the structural traits of experience, including its dynamic characteristics. It was in anticipation of such considerations that in the ninth chapter I gave the concept "force" a frankly dynamic meaning. There is no objective evidence whatsoever on the basis of which the physicists could object to this procedure. They do prefer to have their concepts pale and slender. Such a predilection may, however, have to yield to the demands of scientific situations which open a fuller view of dynamics than can be treated by mathematical functions without a dynamic meaning. From the customary point of view the mere fact

that such situations will arise in the investigation of man and of mental life may appear suspicious. And yet no anthropomorphism is involved in a procedure that makes man the subject-matter of positive investigation, and that adapts our fundamental functional concepts to what is found in this new field, just as it makes them fit the data of ordinary physics. G. E. Müller once remarked that no system of physics can be regarded as final that has not yet been exposed to contact with psychophysical problems. This statement seems to me entirely correct. It points, I believe, to one of the greatest tasks of science.

In the theory of isomorphism one point deserves special attention. One might be inclined to believe that essential traits of mental life, such as relationship in general and requiredness in particular, have neural counterparts which are enormously complicated, and owe their origin to a long evolutionary history. As a matter of fact, however, it would be difficult to apply the concept of isomorphism to those instances if this assumption were correct. Evolution brings forth new histological structures which impose on function new and particular conditions. I do not see how such a development could ever lead to neural processes which would be isomorphic with relationships or with requiredness. If, for example, between two different processes a gradient develops in an approximately homogeneous medium, this gradient as such will adequately represent or functionalize their difference. Any special morphological arrangement in the medium, on the other hand, would be likely to distort rather than to further such a representation. Gradients occur in nature quite apart from any evolution in the biological sense. Evolution could not make them any more isomorphic with relations than they are *per se*. It might be that, during evolution, brain-fields

became gradually more "homogeneous" than they were previously. Even in this case evolution did not create the correlates of relations, i.e., gradients. It merely led to a condition in which such gradients are no longer distorted by additional circumstances

Similar remarks might be made in the case of requiredness and its neural correlate. Some fundamental properties of nature rather than any special arrangements in the organism are, I believe, the counterparts of essential mental facts. I admit that this is not a familiar notion; but it has certain advantages. It excludes the possibility that such mental facts constitute quite particular and to this extent merely "subjective" traits of our organization. For several reasons, it is true, the highest achievements of human mental life presuppose the existence of many special anatomical structures. The basic functions themselves, however, which are involved in those achievements seem to me entirely independent of any such machine-arrangements. As I see it, this fact adds to their dignity and to the objective significance of mental activities.

III

If the physicists tend to divorce man from nature, because they suspect him of adding subjective ingredients to their system, those who are more interested in man than in nature like to do the same, because they are disturbed by the fact that physical, or more particularly organic, events seem to "influence" mental activities. Since we discussed this last point once before—in the beginning of the sixth chapter—I shall here add merely a few remarks.

At the time when I gave these lectures at Harvard I was once visited by a gentleman who was seriously afraid of

what he called my Materialistic tendencies. I will confess that when people begin to discuss philosophical problems in terms of concepts which belong to a past period in the history of human thought, I invariably feel a certain heaviness, and my mental processes become extremely slow. The same happened this time. It was only with a great effort that I could patiently explain why I regard such questions as not particularly important. And no sooner had my visitor left me than I sank into a comfortable chair, in which I must almost at once have fallen asleep. I began to dream. In my dream I was in court, as a matter of fact on trial, and everywhere around there were people whom I seemed to recognize as members of my audience at Harvard. They were obviously there as witnesses, and their faces were distinctly hostile. "Well, Mr. Köhler," said the presiding judge, "do you admit that you have been propagating Materialistic ideas?"—"I don't know, your honor," I answered; "just what does Materialistic mean?"—"Do not try to dodge my question," said the judge severely; "of course you know what *matter* is."—"Unfortunately," I replied, "so far as matter is concerned my training was obviously not what it should have been; they never told me." —"You are trying to play the rôle of the innocent," insisted the judge; "everybody knows what matter is."—"That may be so," I answered; "but everybody knows so many things with which I am entirely unacquainted."—Thereupon they all became very angry with me, and presently they called in a physicist as an expert. He delivered a long and learned address about wave-packets and indeterminacy, about Schrödinger, Heisenberg and Dirac, until they became angry with the physicist, too. Eventually an old man in the audience raised his hand, and when he was allowed to speak he said that far up in Maine, behind a half-forgotten

village there was a huge forest, and behind this forest something like a dark and narrow tunnel in the ground, no less than three miles long. Beyond this tunnel, on the other side, he said modestly, there were people who could probably tell us what matter is. So we were sent to Maine, the expert and I, with some court-officials and not a few guards. We found, indeed, first the village, then the big forest, and, after some exploration, the dark tunnel. Through this we went with some discomfort, because it was narrow, and on the other side we came upon some large white buildings that looked exactly like new Federal Post Offices. One of them had the inscription "Department of Physics." We entered at once, thinking that this must be the right place for our inquiry. At any rate it was a remarkable place. We were not a little surprised when, on being questioned, they told us that here new electrons, protons, fields, and so forth, were manufactured as substitutes for those which were simply worn out by their strenuous occupations. Of course, we wanted to know immediately whether they had some good specimen of new *matter* ready. "Matter?"—the employee thought hard—"I seem to have heard this word before. Let me look at my catalogue." He produced an impressive volume and looked up under M. There it was indeed,—but it had been deleted. "I remember now," said our friendly informant; "years ago we had it in this department. But then they found out that it was here by mistake; that it should never have been here at all. At present, my list says, it is handled in the "Department of Objective Phenomena" and nowhere else."—"Well, I'll be...," the physicist interrupted himself in time; "let us go to that Department, and see what this means." All agreed, and so we went to another building which bore that inscription. Now, here we felt

entirely at home. In the "Department of Physics" they had not shown us a thing; here shelf upon shelf along the walls was simply crowded with lumps of nice, good, inert new matter, tiny and big, heavy and light, of all shapes, and ready for all possible purposes.—The next thing which I remember—it is too bad that afterwards there are always gaps in our dreams—well, I remember that the physicist and I found ourselves sitting on the ground near the half-forgotten village under a fir-tree; none of our guards was in sight, and we felt a bit dizzy. After a while I began to ask the physicist questions in order to see whether he could offer any explanation. "What is an electron?" was my first question. He made it convincingly clear that an electron is a volume of space that resists the approach of more electrons, and attracts protons.—"What are protons?" I asked him. He was quite definite about this, too: "A proton is a volume of space which resists the approach of more protons, and attracts electrons."—"But what is in these volumes?" I wanted to know. "You speak only about dynamics within certain volumes of space; you do not say a word about those things themselves."—"I could not tell you," said the physicist; "nobody knows. Once Schrödinger believed that those particles were wave-packets. At present, however, we think that this was not quite the right interpretation. Moreover, if you were to ask me 'packets of what?' I could once again only fall back upon dynamic effects which do not seem to satisfy you. Perhaps I should add that we know some more properties of electrons and protons. But they are without exception dynamic qualities. The electric charges which we attribute to both electrons and protons are quantities which we compute from their behavior. I cannot tell you what a charge as such is. And, as to the mass of electrons and protons, of course you know

that mass is merely a coefficient of resistance against acceleration, that it has nothing to do with the concept matter."

My questions were caused by the fact that matter is often said to be composed of electrons and protons. But, if matter is a substance, the physicist was obviously unable to clarify my ideas about this substance. So far as he was concerned there might be no such substance at all. And yet, if people are so very much afraid of Materialistic doctrines, there must apparently be something horrid that is called "matter," and that Materialists declare to be the essential principle of the world.—A long silence followed during which we were both hunting for this principle of badness. The more we tried, the more hopeless seemed our enterprise, until we were genuinely depressed. But then I had a revelation. In the "Department of Objective Phenomena" we *had* seen an abundant supply of matter. And had it not been deleted in the "Department of Physics"? Suddenly the meaning of these facts became quite clear to me, and I tried at once to convince the physicist that I was right. "Years ago," I told him, "people did not know that we have carefully to distinguish between thing-percepts and physical objects. At that time a thing-percept was a physical object at the same time; if from the former a few subjective additions were eliminated, the remainder *was* the physical thing. Again, when a Materialist spoke of matter he referred naïvely to peculiar properties of thing-percepts. Thing-percepts, we know, are segregated units in perceptual fields. Now take, for instance, visual things and compare them with a mere background, or with the empty parts of visual space. Those things look as a rule 'dense' and 'substantial'; where they are, there is visually 'something,' there is a 'material.' A mere background exhibits

such properties in a lesser degree; mere visual space does not show them at all, and is therefore called 'empty.' In kinaesthesis and in touch we find the same dualism between 'material' contents on the one hand and mere 'emptiness' on the other. Great philosophers have tried to analyze the meaning of such words as substance and matter. But at the time the fundamental concept of organization was not yet clearly developed; nor were those philosophers acquainted with the fact that in perceptual organization many new properties are created which cannot be accounted for in terms of sense-impressions. It suddenly occurred to me that concepts like 'substance' and 'matter' refer to those properties which segregated things exhibit in perceptual fields, and that in this sense both 'substance' and 'matter' are terms which owe their meaning to facts of perceptual organization. That this is the origin and the nature of matter could, however, not be recognized until psychology and phenomenology had reached a more advanced stage of their development. For the same reason it could not occur to the philosophers that a direct application of this concept to the physical world might not be a safe procedure. On the contrary, in this respect Naive Realism, the identification of percepts with physical things, remained for a long time practically undisturbed, precisely because the perceptual nature of matter had not been realized. Over there, on the other side of the tunnel, however, the administration is no longer deceived. Thus matter has been transferred from the "Department of Physics" to that of "Objective Phenomena." As it seems to me now, this has been a sound measure."

The physicist made some remarks about the psychological part of my explanation which merely showed that he had difficulties in following a phenomenological argu-

ment. He raised, moreover, the objection that, with or without matter as a physical substance, the interpretation of mental facts in physical terms might be called a Materialistic doctrine, and that the usual aversion toward such an interpretation might be wholly defensible.

If I remember correctly my answer was more or less along the following lines: "You forget," I said, "that in common human experience 'matter' shows for the most part a highly characteristic behavior. It is passive in that it needs a support; it 'falls' unless it be supported. Naïve experience is of course entirely unacquainted with any attraction between a piece of matter and the ground as the representative of the physical earth. Thus 'heaviness' appears as a passive property of matter as such. Again, apart from falling, when does matter move? When it is pulled or pushed by something else that comes in contact with it. Here again matter is characterized by its passivity and its inert nature. According to ordinary experience it is more natural for a thing to be at rest than to move. Such experience reveals as a rule no interaction in the sense that fields extend between objects; it regards forces as agents which operate only in one direction; it therefore distinguishes between active and passive constituents of given situations, and it considers matter as essentially the principle of passivity, which is merely temporarily made active by impulses from without.

"Common experience does not know how very limited and, therefore, how misleading its knowledge is. Hence the deep impression which magnetism and electricity make on those who first become acquainted with them. But it was that limited and misleading experience on which the more primitive philosophical opinions about the physical realm were based in the Western world. When Materialistic sys-

tems developed, the same ideas about matter and its essentially passive nature became predominant in these systems. From the Materialist's point of view mental life is identical with or at least a product of certain physical events. If these events consist in forced displacements of passive matter, it follows that our experience of mental life is altogether illusory. Actually mental operations must be just as crude and primitive as all physical facts are supposed to be.

"It is not surprising that this interpretation of mental life met with energetic resistance. Throughout the centuries the opposition was strong, precisely because other philosophers shared for the most part the Materialist's view of physical nature. At the present time we know perfectly well that this view represents a caricature rather than a real knowledge of nature. And yet there remains a strong after-effect of those unfortunate misconceptions, of those earlier disputes, and of the sentiments which then originated.

"What does the term Materialism mean in our time? Let us be quite clear about one point. It is not some particular doctrine or -ism; it is a fundamental postulate of modern science in general that both as an organism and as a mental agent man has developed from lower forms of organization. Ultimately, we are convinced, he descends from inorganic nature. It is again no Materialistic hypothesis, but a well-established fact that between our mental processes and biological facts there exists the most intimate relationship. In this situation I do not see what we can possibly gain by turning our eyes in other directions, by ignoring what may be the most urgent problem of philosophy, and by accusing those of Materialism who face this problem frankly. What seems to be needed in this field is a completely new start. How are we to understand that

intimacy between mental life and neural events? How are we to interpret the fact that mental life originated in the midst of so-called physical nature? If we wish to answer these questions we should first of all try to get rid of an emotional attitude which historically arose from previous, and undoubtedly unacceptable, answers. We should next study nature with impartial zeal, although with special attention to such functional traits as seem to give us some understanding of the psychophysical problem. In this enterprise it might help us if we could altogether avoid certain terms. The term matter, for instance, for which there is no place in physics should not be used in connection with our investigation. But if we could also avoid the words physics and physical, so much the better. They, too, are utterly charged with unfortunate meanings and with corresponding quasi-emotional connotations. What we intend to investigate might be called 'the world of those functional structures which are not directly accessible.' Can anybody object to an investigation which aims at an understanding of such functional structures in their relation to mental life? Few people will object, so long as we do not speak of the relation between *physics* and mental life. So strong is the power of old words and of their historical connotations. What we mean by physics is of course merely those functional structures. They are known to be quite different from the picture of nature which Materialistic philosophers had in mind. It seems possible that with our present *Fragestellung* we shall learn to see them in a light that is to some extent new even to you, the physicists."

I may have dreamt what the physicist replied at this point. What I recall is, however, no more than that, before I awoke, there was some unpleasantness between us.

On looking back upon my dream I feel inclined to believe that I was essentially right in my remarks about the nature of matter. I realize that, from the point of view of isomorphism, the perceptual entity 'matter' involves further problems. But, since it seems obvious that its neural correlate must be some form of cortical *function,* I feel quite satisfied that these problems cannot lead us back to anything like Materialistic conceptions.—I am also convinced that in a threatening philosophical predicament it is not only more courageous but also actually preferable to face the dangerous situation squarely rather than to avoid the issue with one excuse or another. As I said once before, the philosophical theories of the last hundred years have tried to escape where no escape is feasible. Vitalists have attempted to separate the organic realm from inanimate nature, and to defend the former against the principles which govern the latter. Other philosophers soon began to realize how useless any resistance must be in this outlying territory. They retreated into the realm of the mind and of human culture. But Naturalism penetrated into this stronghold, too. Thus Husserl sacrificed the whole world of facts, including those of actual mental life, and took refuge in a world of pure timeless essences. It has all been in vain. The psychophysical problem, the intimacy between mental life and biological facts, gives the enemy such a strong position that any attempt to escape is a hopeless endeavor. Even timeless truth is known only in actual mental events which as such are subject to the same threat.

I do not see a single position to which we can now retreat. Under these circumstances I propose that we dismiss our fears, turn around, and take the offensive. It is true, the association of mental life with biological facts is intimate. But is this intimacy as threatening as it is generally

believed to be? As I see it, we should quietly accept it as a fact and then examine the alleged danger. May I repeat: No connection with anything else can change the characteristics of mental operations. With this premise the intimate relation between mental life and certain facts of nature can mean only one thing. These facts of nature have to be interpreted in a way that makes them compatible with our experience of psychological facts. It is a bad habit to believe that in the nature of the psychophysical problem there is contained a threat to the characteristics of our mental processes. What we ought to say is that from the intimacy between mental operations and their cortical counterparts there follow certain conditions which these neural correlates and, with them, other parts of nature have to satisfy. If we find that with the present views of natural science these conditions can be fulfilled we shall be contented, and we shall then surely have no reason to fear the contact with nature. On the other hand, if at certain points the present system of science seems incompatible with those conditions, we shall *insist* upon the urgency of the psychophysical problem, we shall *emphasize* the intimacy between mental life and nature, and we shall calmly ask for such changes in the conceptions of science as will make them fit our conditions. In this case again, why should we be afraid of nature and of science?

In these ten chapters I have tried to take a few steps in the new direction. So far no serious difficulty has arisen. Natural science does seem able to satisfy our conditions, provided that we occasionally give its views a slightly modified formulation, and provided that we lay some stress on principles and on concepts to which the scientists pay less attention. Requiredness was the most important notion within the realm of mental facts that appeared threatened

by "Naturalism." When we turned around and used the principle of isomorphism as our most effective tool or weapon, it soon became evident that to careful inspection nature reveals the same dualism between mere facts and selective tendencies as that which characterizes mental life.

I admit that in this essay we have not achieved much. Mere requiredness as such occurs within the mental processes of insane and of feeble-minded people. On the other hand, mental life at its best exhibits a consistency, a coherence, and a hierarchy of values which we have left entirely undiscussed, among other reasons because the strictly psychological and philosophical investigation of this field has not yet reached the stage in which it would be fruitful to consider the corresponding psychophysical problems.

In the meantime our first tentative steps seem encouraging. Even the editor with whom I conversed in the first chapter might now see some possibilities of which he did not think at all when he complained about the inability of science to deal with man and with questions of value. I should expect him to express impatience at the fact that our analysis does not give him much help in his essential human affairs. I share his impatience; but I do believe that we have now overcome some difficulties of principle, and that our further advance will be accordingly facilitated.

People like to subsume under some familiar category what they read. It makes the intellectual situation simpler. In this sense the reader might ask whether our investigation does not lead to a Monistic interpretation of the world. If structurally and functionally the neural counterparts of mental processes are isomorphic with these processes, why

should we speak of two different things? Why not identify the former with the latter, or vice versa?

My answer would be another question. What advantage, I should say, could we derive from this simplified formulation? It may be that some such Monism will sooner or later appear as the natural view in this field. I am sometimes inclined to think so myself. But at the present time the theoretical situation does not seem ripe for such a step. On the contrary, we ought to be wary of any over-simplification. Accepting Monism as a philosophical doctrine, we should at once be compelled to discuss its import in the realm of inanimate nature. I do not feel in the least prepared for such a bold adventure. As to the psychophysical problem, moreover, phenomenological observation is one procedure, and the construction of isomorphic correlates, *plus* their hoped-for demonstration in physiological work, is an entirely different procedure. So long as isomorphism remains an hypothesis which will have to undergo one empirical test after another, it would merely confuse our own thinking if we were to anticipate a positive outcome of all these tests, and to undertake further speculations on such an insecure basis. I pointed out once before that, as the term is commonly used, I cannot regard Monism as a clear doctrine. It may become sensible precisely to the extent in which isomorphism can be shown to constitute scientific truth. Why not wait until this factual problem is at least partly solved?

As a further argument in favor of reserve and caution I should like to mention the fact that, after all, the cortical correlates of mental facts have a microscopic, an atomic, phase no less than they have macroscopic properties. In the relation between those microscopic facts and certain phenomenal data the most serious problems are involved. For

the moment I see no possibility of applying the concept of isomorphism to, say, phenomenal color on the one hand and microscopic chemical events in the brain on the other hand. Monism in the historical sense is not disturbed by such difficulties; it postulates an identity even though in experience there is a striking dualism. I have no interest in any Monism of this kind. Colors and many other phenomenal qualities appear to me as different from all the microscopic and macroscopic processes with which the physicists deal. Why, then, should we conceal this fact behind the name of a metaphysical doctrine that expressly denies the fact as such? If we did adopt this name it would merely make us ignore a particularly intriguing problem of natural philosophy. To be tranquilized by the familiarity of a term is not a commendable attitude. The present writer would prefer to have as full a view of the psychophysical and of related problems as he can possibly obtain. For this reason he also feels justified in ending this investigation at a stage in which there are few answers and countless questions.

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